

THE DEVELOPMENT OF STATISTICAL THEORY IN BRITAIN, 1865-1925:
A HISTORICAL AND SOCIOLOGICAL PERSPECTIVE

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DECLARATION

This thesis has been composed by myself and
the research on which it is based was my own
work.

Abstract

This thesis discusses the development of statistical theory in Britain in the period 1865 to 1925, and attempts to account for this development as an institutional and an intellectual phenomenon. Close connections are shown to have existed between statistical theory as a scientific specialty and eugenics and social Darwinism, in particular in the work of Francis Galton (1822-1911) and Karl Pearson (1857-1936). An analysis of eugenics as a social and political movement is presented, and it is argued that eugenics played a major role in facilitating the institutional growth of statistical theory as a field of study. Two scientific controversies involving Karl Pearson and his followers (with William Bateson and the early Mendelians, and with George Udny Yule) are examined, and it is suggested that these controversies might usefully be seen as generated and sustained by divergent social interests. The development of the theory of statistical inference in this period is discussed briefly, and the early pioneering work of W.S. Gosset ('Student') and R.A. Fisher is surveyed.

It is concluded that the generation and assessment of scientific innovations by statisticians in this period must be seen as fundamentally affected by social factors having their origins both within science and in the wider society.

Preface

The bulk of my research on the development of statistical theory was supported by a scholarship from the Carnegie Trust for the Universities of Scotland. A large number of individuals helped me in my research and in the production of this thesis and of the articles listed below. I should like to thank the following in particular: Terri Ainslie, David Bloor, Alan Cock, Goeff Cohen, Ruth Schwartz Cowan, David Edge, Lyndsay Farrall, David Garnett, Richard Garnett, Dave Gibbens, George B. Greenwood, Jon Harwood, Jan Keen, Helen Rugen, Steve Shapin, Oscar Sheynin, Sue Simmons, Professor E.S. Pearson, Gary Werskey, Brian Wynne. Bernard Norton provided me with many hours stimulating discussion of the topics discussed here, and my supervisor, Barry Barnes, was an invaluable guide and mentor in the theoretical issues of the sociology of scientific knowledge. Any errors and shortcomings are, of course, my responsibility alone.

Four articles based on the material presented here have already been published or are in press: MacKenzie (1976), MacKenzie (1977), MacKenzie and Barnes (1975) and MacKenzie (1978). These correspond roughly to chapter three, part of section 5.1, sections 6.1 to 6.5 and chapter seven, respectively, of this thesis.

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Chapter One

Introduction

1.1 Scope and Sources

The aim of this thesis is to analyse a crucial period in the development of statistical theory in Britain, in an attempt to account for this development as an institutional and intellectual phenomenon. The primary focus is upon interpretation and not upon narrative or description. Although ascertaining 'what actually happened' remains of the highest importance, it is more as a means to an end than as an end in itself. Matters of historical and sociological explanation are central.

The first priority is, nonetheless, that of delineating the topic to be studied and indicating the available sources and materials for that study. The years 1865 and 1925 are of symbolic importance rather than definitive boundaries between historical periods. In 1865, Francis Galton published his first paper applying quantitative methods to the study of heredity. Before that date, and for a good while after it, it would be misleading to talk of the existence of a scientific specialty called statistical theory. There were of course a number of isolated pieces of work that can be pointed to as contributions to the theory of statistics. These did not, at/...

at any rate in Britain, add up to anything approaching a coherent tradition of work.⁽¹⁾ By 1925, the date of publication of R.A. Fisher's Statistical Methods for Research Workers, many of the intellectual and some of the institutional foundations of modern statistical theory had been laid. The development in the intervening period of the 'British school' of statistical theory is thus an important episode in the history of modern statistics.

I shall make no attempt to provide a rigorous definition of 'statistical theory'. By use of the term I wish simply to distinguish the object of this study from, on the one hand, the activity of gathering quantitative information typically engaged in by official bodies and social scientists, and, on the other hand, the mathematical theory of probability. Of course, statistical theory (or mathematical statistics as some might prefer to call it) is not separated from these fields by an impermeable boundary. It is, however, a distinct enterprise. The individuals dealt with here certainly collected numerical data, and were acquainted with, even if they did not work on, the mathematical theory of probability. They were, however, most distinctively involved with the construction of a theoretical framework for the analysis of quantitative data. They developed new concepts, such as correlation, regression/...

(1) It could perhaps be argued that in the mathematical specialties of probability theory and the theory of errors there were coherent traditions. Aside from the issue of how much of this work can be counted as statistical theory, it should be noted that, as discussed in section 2.1, these specialties were largely Continental rather than British in their composition.

regression and likelihood, that allowed data to be analysed in new ways. These concepts were formulated mathematically, but much more was involved than simply the application of existing mathematical theory to a new field. Shape was being given to a distinctive set of techniques of data analysis. These techniques, and not quantitative data nor probabilistic mathematics, form the primary subject matter of statistical theory.

The major source for this study is of course the scientific publications of British statisticians of the period, the books and papers of men such as Francis Galton, Karl Pearson, R.A. Fisher, F.Y. Edgeworth, George Udny Yule and W.S. Gosset ('Student'). Here we find the explicit development, presentation and justification of their innovations. As the aim of this study is not simply a description of these innovations, other sources of evidence were also drawn upon. Many of the men discussed here wrote widely on subjects other than statistical theory, and these writings have been used to gain information on their more general intellectual stances. A large body of archival material, chiefly letters between major participants in the development of statistical theory in Britain, was consulted. A list of these archival sources will be found at the end of this thesis (appendix A). This material helped fill in gaps in the written record, and provided crucial information about the attitudes of those, such as George Udny Yule, who did not publish widely on non-statistical topics. Several individuals/...

individuals also provided me with information, in interview or by letter, about this period. As most of the events discussed here took place well over 50 years ago, this information did not consist of eye-witness accounts. Nonetheless, my informants suggested useful lines of inquiry and provided interesting insights into certain aspects of the personalities and developments discussed below. Finally, of course, there is a range of available secondary sources, from obituary notices written by contemporaries to the work of professional historians. These are discussed in the following section.

1.2 The Historiography of British Statistics

The historian of British statistics has the good fortune to have two excellent general bibliographic sources. The volume of Kendall and Doig's Bibliography of Statistical Literature that deals with the period before 1940 (Kendall and Doig, 1968) provides a remarkably comprehensive list of papers and books on statistical theory and method. The construction of the bibliography has evidently not been restricted by an anachronistic notion of statistical theory, and an extremely wide range of periodicals has been searched. Although small errors naturally creep in to a work of this nature,⁽²⁾ its existence saves the historian of statistics many hours of work. H.O. Lancaster's Bibliography/...

(2) For example, the paper by Black (1898), discussed in chapter five below, is mistitled. However, the very inclusion of this paper, unknown to historians, indicates the value of the bibliography. The most serious omission I have noticed is that of Galton(1877).

Bibliography of Statistical Bibliographies (1968), annually updated and extended in the Review of the International Statistical Institute, provides a very useful list of biographies of statisticians (generally obituary notices). Aside from the useful biographical information these latter contain, in many cases they provide bibliographies of the works of the individuals concerned which are, for example, useful in pointing to non-statistical publications not listed by Kendall and Doig.

The volume and range of the writings of the three most important figures of this period of British statistics, Galton, Pearson and Fisher, present unusually severe problems for the bibliographer. These problems were largely overcome for Galton by Karl Pearson (1914-30) and Forrest's revised list (1974, 303-17) must now be considered definitive. Morant's 648-item bibliography of Karl Pearson's writings (Morant, 1939) greatly aids work on Pearson; some additional items are listed in Eisenhart (1974). Bennett's edition of the papers of Fisher (Bennett, ed., 1971-4) contains a bibliography of all Fisher's papers, not simply those reproduced.

There is a considerable quantity of historical writing on British statistics to be consulted. General histories of statistics are of fairly limited usefulness. The two best and most recent are Walker (1929) and Westergaard (1932). Westergaard's book deals well with such topics as official statistics in the nineteenth century/...

century, but the account ends in 1900 and thus the treatment of the developments discussed here is summary.

Walker (1929) is still useful as a reference work. Her book does discuss the work of this period, although in a descriptive rather than analytical fashion.

Much more important are the historical works written by members of the 'British school' themselves. Karl Pearson's massive study of Francis Galton (K. Pearson, 1914-30) has been criticised by Galton's latest biographer as resulting in the 'burial of the man beneath the monument' (Forrest, 1974, ix). While it is true that its bulk must certainly deter the general reader, Pearson's work remains a vital source of information for the historian: for example, many of the most important letters to and from Galton are reproduced therein. E.S. Pearson's biography of Karl Pearson (E.S. Pearson, 1936-8) lacks the all-encompassing nature of its subject's biography of Galton, but it is still a major source of information on Karl Pearson. E.S. Pearson's other historical work, notably E.S. Pearson (1939; 1965; 1967; 1968), forms, together with his biography of Karl Pearson, an invaluable account of the development of statistical theory in Britain. Although this work is largely of a descriptive nature, it contains important explanatory insights. E.S. Pearson's biography of 'Student' (E.S. Pearson, 1939), which draws on the unpublished reports of 'Student' to his employers to reveal the importance of his situation as an industrial scientist/...

scientist in conditioning his statistical work, is perhaps the most important of these studies, and has been made use of here in chapter eight.

Many other writings by statisticians and historians of science have also been drawn upon. The series of 'Studies in the History of Probability and Statistics', published in Biometrika, is the most important single set of papers. Several of these have been reprinted, together with other relevant material, in E.S. Pearson and M.G. Kendall (eds) (1970) and M.G. Kendall and R.L. Plackett (eds) (1977). The bulk of work on the history of statistics deals with the period prior to the nineteenth century, and thus is of little direct relevance here.⁽³⁾ Some writers have, however, dealt with the period under discussion here. Thus Weiling (1969) gives an overview of developments in this period, while Wei-Ching Chang (1973) focuses on one particular topic, the development of the chi square test.

The work with which this thesis must most directly invite comparison is, however, that of five historians or sociologists of science who have dealt with the topics discussed here: Joseph Ben-David, Ruth Schwartz Cowan, Victor/....

- (3) The most systematic work in this area is that of the Soviet historian Oscar Sheynin (1966; 1970; 1971a; 1971b; 1972a; 1972b; 1973a; 1973b; 1974; 1976). Amongst other works worth particular attention are the studies of Gillispie (1972), Baker (1975) and Buck (1977), which focus on the 'social relations' of statistics rather than exclusively on its intellectual development.

Victor Hilts, Lyndsay Farrall and Bernard Norton.

Ben-David's brief treatment of the development of statistics (Ben-David, 1971, 147-52) is interesting because it forms part of a work on the sociology of science which employs a different perspective to that of this thesis. Much of the material presented here can be taken as confirming Ben-David's discussion of the institutional development of statistics in Britain. My conclusions on the intellectual development of statistical theory, however, prove to be sharply at variance with the view of science put forward by Ben-David: see below, sections 1.3 and chapter nine.

No such sharp disagreement exists between the conclusions reached here and those of the other four authors. The work of Cowan on Galton (Cowan, 1972a; see also Cowan, 1972b and Cowan, 1977), which emphasises the key role of Galton's eugenics in motivating his work on statistical theory, is fully confirmed in its central argument. Here I am attempting to build on Cowan's work in two ways. Firstly, the relationship between statistics and eugenics is examined, not only for the vital case of Galton, but also for later statisticians, in particular Karl Pearson. Secondly, while Cowan's focus is primarily on the question of motivation, the focus here will be more on the connections, if any, between factors such as eugenics and the content of statistical theory and statisticians' evaluations of it.

Hilts (1973) places, by comparison with Cowan, more emphasis on the question of the content of Galton's innovations/...

innovations, and less on the question of motivation. Although he does not focus as clearly as Cowan does on the specific role of eugenics in affecting Galton's statistics, he puts forward an interesting argument about the relationship between Galton's theoretical innovations and the work of the preceding tradition of the theory of errors. Hilts's work is drawn on in chapter two, and section 2.4 attempts to formulate more precisely the comparison of Galton's work with that of the error theorists by looking at those error theorists, not discussed by Hilts, whose work can be seen as closest to that of Galton.⁽⁴⁾

The focus of Farrall (1970) is on eugenics, not statistics, but in view of the intertwining of the two areas in the persons of Galton and Pearson, Farrall deals with several aspects of the development of statistics. He gives a very useful account of the 'biometric school' of British statisticians led by Karl Pearson. Chapter three of this thesis, which deals with British eugenics, draws heavily on Farrall's work, which is discussed in more detail there.

The early work of Bernard Norton (1971) deals with the biometric school. As it is largely descriptive, it adds/...

(4) Two other works by Hilts are of less relevance here. Hilts (1967) is a largely descriptive account of the development of British statistics, while his perceptive study of Farr (Hilts, 1970) falls somewhat outside the scope of this work.

adds only in historical detail to that of writers such as E.S. Pearson. Norton's later work, in particular Norton (1975a; 1975b; 1978), goes beyond this to develop an explanatory perspective on several of the developments dealt with here. The conclusions reached here are largely in agreement with those of Norton. On some particular issues, notably the controversy between the biometric school and the early British Mendelian geneticists, differences of opinion do, however, exist. These are briefly discussed in chapter six.

Certain parts of this thesis move outside the history of statistics proper, and in these parts, chiefly chapters three and six, other bodies of writing are drawn upon. The discussion of eugenics makes reference not only to the work on eugenics by Farrall (1970) and, more recently, Searle (1976), but also to more general work in social history, in particular Annan (1955), Hobsbawm (1968) and Gareth Stedman Jones (1971). The biometrician/Mendelian controversy, discussed in chapter six, has become a focus of attention for historians of biology, and their work on it is listed in that chapter. Particular reference should, however, be made to the work of William Coleman (1970) on the biometric school's main opponent, William Bateson. Although Coleman's analysis of Bateson as a 'conservative thinker' lies within the tradition of the history of ideas rather than the sociology of knowledge perspective employed here, his work, if it survives detailed criticism from/...

from historians of genetics, provides an insight into a form of thought radically different from that of the biometric statisticians who are the central focus of this study. Coleman's work does not deal explicitly with the controversy between Bateson and Pearson, but it will be argued that its central thesis can throw useful light on it.

1.3 Is Scientific Knowledge a Social Phenomenon?

Statistical theory in Britain was developed by a group of individuals whose intellectual interests and commitments were in many cases extremely wide. The central figures of this study, and several of the more minor figures, had strongly-held political and philosophical beliefs, and were involved in debating issues of considerable ideological importance. The question then raises itself: should their science be seen as separate from the broader context of their thinking and their social and political views, or should it be seen as intertwined with these?

The rise of British statistics belongs to a period in the development of science described by Ravetz (1973) as 'having its terminal points in the French Revolution and in the atomic bomb'. This was a crucial era for the development of science:

The institutions and attitudes of science and scientists at the present time are largely inherited from that period, and the memory of that era is a point of reference for all analyses of the present except for those which see science simply as a factor of production.
(Ravetz, 1973, 37)

Prior/...

Prior to this period, science was a socially marginal activity, often delineated unclearly from fields such as philosophy and magic. After this period, large sectors of science have become, as Ravetz puts it, 'industrialised' through the massive intervention of the state and business. In the period in question, science became firmly established in the university systems of the advanced countries. Particular disciplines and specialties developed institutional forms for the recruitment and training of professional scientists, for publishing research work and for maintaining control over its quality. While the financial support for science was by no means as generous as it was to become after 1945, this support was typically obtained from sources that placed a minimum of direct constraint on the work of scientists. The notion of pure science as conducted by an autonomous community of individual scholars should therefore apply to this period, if it is to apply to any.

This period is thus a particularly crucial one for those who seek to understand the relationship between scientific knowledge and the wider society and culture in which the activity of science is pursued. Two viewpoints on this issue can usefully be contrasted. The first, widely held by historians and sociologists of science, is that science as an activity is indeed influenced by the society in which it takes place, the degree of social support, for example, determining the pace of scientific advance/...

advance. But, as Ben-David puts it, 'the basic concepts and the logical structure of science' are affected by the wider society in only an extremely limited way. 'Ideological bias' can lead science into 'blind alleys', but true scientific development is determined in its intellectual structure by 'the conceptual state of science and by individual creativity-and these follow their own laws, accepting neither command nor bribe' (Ben-David, 1971, 2, 11, 12).⁽⁵⁾

Thus, in discussing the emergence of modern statistics, Ben-David isolates various social and institutional factors affecting the development of the specialty in Britain and the United States. The rapid development of statistics in America is attributed by Ben-David to the responsiveness of American universities to practical needs. Britain's universities were not responsive in this way, but there was a 'functional equivalent' in the 'semiformal and informal networks and circles comprising the academic elite and outstanding researchers and intellectuals outside the academic field' (Ben-David, 1971, 151). The fact that Britain had, by comparison with the United States, 'a far more developed scientific tradition at the time and a less abstract school of mathematics' (150) is taken by Ben-David to explain the fact that theoretical advance/...

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- (5) The philosophical work of Imre Lakatos, who argues that 'external history' explains only those intellectual developments not accountable for in terms of a 'rationally reconstructed' internal history (Lakatos, 1974), can be seen as broadly parallel to this first position.

advance was much faster in Britain than across the Atlantic. The 'eugenics movement' is seen as giving rise to 'interest in biostatistics' (151). In Ben David's work the structure of social institutions, in particular universities, and the existence of social movements such as the eugenics movement, are thus taken as explaining the rate of advance of statistics. These factors could hinder or promote work in the field and perhaps condition the quality of work done. They are, however, not taken by Ben-David as explaining the content of the theoretical advances. In the light of his earlier general statements, it must be presumed that he considers this content to have been unaffected by them.⁽⁶⁾

The alternative viewpoint rejects the claim that the esoteric knowledge-generating activity of scientists can be understood entirely in terms of 'its own laws', or general norms of rational thinking, or indeed in any terms which effectively bound it off a priori as an intellectual activity to be studied in isolation. If this view is correct, then neither the social organisation of science itself nor the wider society and culture can be excluded as possible determinants of scientific thought and activity.

On this view, at least in its fully developed forms, it is not merely the generation of new ideas within science which is considered to be open to influence in this/...

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(6) Certainly there is no evidence that Ben-David believes that the eugenics movement led statistics into a 'blind alley'.

this way. The evaluation of new ideas by the scientific community and the process of the justification of innovations are also believed to be essentially social. If the term 'social' is taken as referring both to the wider society and to the community of practitioners, then on this second view social factors are assumed to have potential influence on all scientific judgments, not merely 'wrong' or 'biased' judgments.⁽⁷⁾ The best known work which treats scientific activity as essentially communal and constitutively bound up with the social organisation of scientists is of course that of T.S. Kuhn (1970). Concrete expositions of thoroughgoing links between scientific thought and evaluations and wider social and cultural factors have been offered by, for example, R.M. Young (1969) and P. Forman (1971). Barnes (1974) expounds this general position and explores some of its implications.

An attempt will be made here to deal with the detailed content of statistical theory, and with the scientific judgments of statisticians within their specialty. It is hoped that the result will be the presentation of material of relevance to the assessment of the validity of these two contrasting views, in particular with regard to the/...

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(7) If this were a thesis on the philosophy of science it would be necessary to comment on the view that social factors affect the 'context of discovery' but not the 'context of justification'. This distinction is, however, not one that can easily be upheld in concrete historical discussions such as the following. 'Discovery' and 'justification' are inextricably intertwined.

the role of 'external' influences on science.

An examination of the influence of social factors upon statistical theory does, however, pose problems which are in some ways different from those posed by similar work in the natural sciences. Nowadays the theories of the natural sciences are more readily accepted as 'inventions' than are mathematical theories, which are still frequently regarded as 'discoveries'. The propositions of logic and mathematics are sometimes held to have a status quite different from those of the natural sciences. They are held to have the character of eternal truths, to be descriptions of objects in an immutable world of thought. This 'Platonist' or 'realist' view has been advanced, for example, by G.H. Hardy:

I believe that mathematical reality lies outside us, that our function is to discover or observe it, and that the theorems which we prove, and which we describe grandiloquently as our 'creations', are simply our notes of our observations.

317 is a prime, not because we think so, or because our minds are shaped in one way rather than another, but because it is so, because mathematical reality is built that way.

(Hardy, 1967, 123-4, 130. Quoted by Bloor, 1973, 176. Hardy's emphasis.)

If the realist view of the propositions of mathematics and logic is adopted, then it is clear that to the extent that statistical theory is mathematical and logical in its nature, an a priori negative answer is provided to the question of whether social factors can affect/...

affect the content of statistical theory. For if the propositions of statistical theory are 'discoveries' and not 'inventions', the role of social explanation is limited and cannot include explanation of the content of theoretical developments. The history of statistics would then have to be written as we currently write the history of geographical exploration. In the latter, we might perhaps explain in terms of social factors the timing of Columbus's voyage, why he set off on the course he did, and why he was mistaken about what he found. We would not, however, explain in this way the content of what he found. America we would regard as being there whether or not he found it; its physical features we would treat as independent of his activity. If we think of Galton as 'discovering' correlation, and of correlation being an eternally existing mathematical-logical object, then we cannot hope to be able to provide a sociological explanation for the content of Galton's work, whatever we might be able to say about his motivation, the timing of his work, and so on. ⁽⁸⁾

There/...

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- (8) In fact, the parallel on a realist theory of statistics to Columbus would not be Galton, but Bravais, who might on this view be said to have 'discovered' correlation, but not to have realised he had done so. Walker (1929, 96-8) comments:

... it is known that [Bravais] set forth the mathematics of the normal correlation surface three decades before the idea of correlation had been conceived ... Bravais recognised the existence of a relationship, a 'correlation', between his principal variables, but gave it merely passing notice ... [he] remained unaware of the stupendous idea in whose vicinity his mind was hovering ... he might, with one leap of creative imagination, have pounced squarely upon this conception ...

For an alternative view of Bravais, see section 2.4.

There are, however, good reasons for doubting the realist view. As a theory of mathematics it has been convincingly challenged, for example by Wittgenstein (1967; see Bloor, 1973). Wittgenstein exposes the lack of any justification for supposing that real mathematical objects stand in correspondence to our mathematical concepts and procedures. Bloor emphasises the tendentious teleological character of accounts of mathematical innovation that 'explain' thought processes in terms of their own products. Moreover, whatever plausibility realism possesses appertains only where there is broad agreement upon the validity of some piece of mathematics, or mathematical procedure. Such agreement does exist in the case of, say, the theory of correlation. But many areas of statistical theory exhibit not consensus but fundamental disagreement; for example, the theories of estimation and hypothesis testing. In discussing these areas, we would not know which of the competing 'real worlds' of statistical theory to take as the basis of a realist account. In pondering areas such as these, it is hard to avoid the conclusion that the 'real' status of concepts such as correlation arises simply from the consensus about them and their consequent taken-for-granted nature. (9)

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- (9) Of course there are good reasons for the consensus about correlation, but the point is that this consensus is surely better seen as the result of the activities and discussions of statisticians, and of their experiences using the concept, and not as a consequence of the ontological status of the concept itself.

This thesis will eschew realism and will talk of statistical 'inventions' rather than statistical 'discoveries'. There are good reasons for doing so, such as avoiding the a priori closure of interesting lines of inquiry, but fundamentally this can only be a presupposition. A realist history of statistics would probably emphasise the 'purely mathematical' aspects of statistical theories, the fact that much of statistical theory can be presented as simply the study of the properties of a set of functions of the real numbers such as the normal distribution, chi square distribution and Poisson distribution.⁽¹⁰⁾ In accord with my anti-realist prejudices, I shall by comparison place emphasis on statistical theory as the construction and study of techniques of reasoning. On this latter view, the correlation coefficient is taken, not simply as a parameter of a particular bivariate function, but as a means for analysing data in a particular way to allow particular conclusions to be drawn. In short, this thesis will treat statistical theory not as a set of truths but as a set of tools for thought.

Treating statistical theory as a body of inventions makes it possible to ask two questions that do not arise for the realist. What are these inventions made out of/...

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- (10) It is probably this, together with the taken-for-granted status of the mathematics of real functions, which underpins the ease with which historians of statistics can slip into realism.

of? What are they made for?

The history of statistics (as indeed the history of culture generally) provides a clear answer to the first question. New theories and techniques are produced by the extension, adaptation and modification of existing theories and techniques and by the 'borrowing' of ideas from other areas of culture. New ideas do not emerge from a vacuum, but are built from existing 'cultural resources' (Barnes, 1974, 45-68). This, of course, is one reason for the 'continuity' of British statistical theory pointed to by E.S. Pearson (1967). Even striking innovations, such as (at least on the view taken here) Galton's notions of regression and correlation, were, as will be shown in chapter two, constructed by the metaphoric extension of biological ideas, and not developed from nothing.⁽¹¹⁾

The/...

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- (11) This general point, which is indeed obvious to historians of science, has interesting consequences for the sociology of knowledge that are sometimes overlooked in theoretical presentations of it. It becomes clear that there is no contradiction between asserting, on the one hand, the cultural continuity of a given intellectual field and, on the other, the role of social interests in shaping developments in the field. Both can be the result of the creative activity of human beings in shaping, from their existing stock of knowledge and cultural achievements, new concepts adequate to the demands of new situations. While social interests may be a necessary cause of intellectual innovation, they can never be a sufficient cause. This implies, for example, that the attempt by Lukács (1971) to deduce from the interests of a given class the form of a unique appropriate 'imputed class consciousness' is doomed to failure. The effects of pre-existing culture can never be ignored. The necessary corrective within the Marxist theory of knowledge to Lukács is Gramsci (1971), with his sensitivity to the role of pre-existing culture in the formation of class consciousness.

The question of 'what are these inventions made for?' is rather more difficult. The dilemma is this: whenever techniques, procedures and concepts are developed and assessed, the context of assessment is potentially broader than that defined by their particular intended applications; simple-minded instrumentalism is unsatisfactory as a means of understanding scientific and mathematical innovation and judgment. On the other hand, the assessment and judgment of innovation apparently always takes place in a way that gives prominence to certain concrete and specific aspects of the efficacy of innovations. Historians and philosophers of science have been unable to find actually operating within science a purely abstract, context-independent means of assessment that does not make reference to the particular achievements made possible by the innovations being assessed.⁽¹²⁾

In what follows I shall enquire to what extent statistical techniques were actually constructed and evaluated with reference to the potential they were thought to possess for solving particular problems. I shall hypothesise that the judgments of the statisticians to be studied were in some as yet undetermined degree goal-oriented.

A vocabulary is needed to give expression to this hypothesis/...

(12) See Kuhn (1970); acceptance of this point is of course by no means confined to the work of Kuhn.

hypothesis. It is perhaps provided by Habermas's notion of 'knowledge-constitutive cognitive interests' (Habermas, 1972). I use the term only tentatively, and certainly do not intend to imply the a priori validity or applicability of Habermas's overall theory. The notion of 'knowledge-constitutive cognitive interests' (or simply 'cognitive interests') will be used here to focus on the process by which particular potential scientific applications or uses of statistical theory may become important factors in statisticians' construction and judgment of theories. Crudely, the term will be used to identify (if they exist) those aspects of the potential applications of statistical theory that 'feed back' in an important way into theoretical development. To put it another way, the term will enable me to discuss the relationships between theory and practice in science by sorting out which aspects of the applications of scientific theory are, from the point of view of the development of that theory, incidental, and which play a constitutive role in theorising and the assessment of theories.

A relatively familiar and straightforward example may help clarify this notion: the theory of errors, developed in the late eighteenth and nineteenth centuries, and widely used to this day in the analysis of the results of observation and experiment. How can one best analyse the construction and evaluation of innovations by error theorists? It does not seem plausible to see error theorists as seeking simply to develop formally valid mathematical syllogisms. Rather/...

Rather, they should surely be seen as judging knowledge claims within their specialty as contributions to a particular way of developing the potential of scientific prediction and control. It was generally acknowledged that experiment and observation could not yield exact, true values of the quantities being measured, but were always subject to a degree of error. One way of improving scientific prediction was to improve experimental and observational technique; another was to use the fact that measurements could usually be repeated in order to control and predict the magnitude of errors in measurement. The goal of error theory can be seen as the construction of mathematical techniques to improve scientific prediction in this latter fashion. Thus, techniques such as the method of least squares were designed and judged as ways of mastering error by providing a 'best' estimate of quantities subject to error. The 'probable error' of estimates measured the reliability of these estimates and permitted the prediction of the likely range of the true value of the quantity being measured. In short, error theory can be said to reflect knowledge-constitutive cognitive interests in improving prediction and control by the mastery of experimental and observational error. Innovations in error theory were judged not in abstract terms alone (for example, as formally correct or incorrect mathematical analysis) but according to their potential as resources in the attempt to master error. If an innovation had no such/...

such potential, it would presumably be judged either as outside the scope of error theory or simply as wrong. (13)

Several points need to be made about the term 'cognitive interests'. Its use does not imply a claim about individual motives, but only about the general character of evaluation within communities of scientists. In any social group there exists the possibility of a disjunction between group norms and the motives of individual actors in obeying these norms. Error theorists, taken as a community of practitioners, can reasonably be seen as constructing and evaluating techniques in terms of their usefulness in improving prediction by the mastery of error. But individual mathematicians may well not have had this as their goal. An individual could after all contribute to the mastery of error by a piece of mathematical theorising that was motivated simply by the desire to gain status by the display of mathematical competence. Nor is it suggested that scientists are necessarily conscious of the cognitive interests informing their work. In the case of error theory, this probably was the case: the very name of the specialism suggests it. In other cases, scientists may well be unaware of, or take as natural and general, cognitive interests that the historian will be able to identify and will see as particular and specific.

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- (13) I hasten to point out that this rather banal analysis is not put forward to throw light on error theory but merely to illustrate what I mean by 'cognitive interests'. Error theory itself is further discussed in chapter two.

Finally, and emphatically, there is no suggestion that scientific specialties are necessarily utilitarian in the ordinary sense of this term, or that all science is 'applied science'. A knowledge-constitutive cognitive interest in prediction and control is quite different from a particular interest in the solution to a specific practical problem. Even in the relatively 'applied' field of error theory there is no indication that particular practical applications as such played a constitutive role in the assessment of innovations. Certainly the general interest in prediction and control was concretised round the specific problem of the mastery of error. This was, however, not a particular practical problem but a common feature abstracted from a wide range of instances of scientific experiment and observation. No single one of these instances took on any special theoretical role.

It will therefore be suggested here that it may perhaps be useful to see innovations in statistical theory (and, in chapter six, in biology) as being constructed and assessed in terms of specific knowledge-constitutive cognitive interests. What might account for the existence of these specific interests? Possibly they are socially sustained and relate to specific social interests.⁽¹⁴⁾ In

(14) To avoid any possible confusion it is perhaps worth giving a rough but serviceable definition of a 'social interest'. I talk of a group as having a social interest in a particular outcome (the acceptance of particular ideas, the development of a particular technique, or whatever) if the wealth, status, power or security of the group would be increased, or other needs of the group satisfied, by that outcome.

Complications of course arise if an attempt is made to distinguish between 'real' and 'perceived' interests, but that is not of primary importance here.

many cases these social interests will arise from within the social structure of science itself. Thus, the cognitive interests manifested in the development of error theory can presumably be related to the communal interests of schools of astronomers and physicists. On the other hand, there may be instances where particular cognitive interests are sustained by social interests arising from the wider society.

The notion of 'cognitive interest' is thus put forward here as a tool for the analysis of the theoretical content of a particular science. By seeking to relate cognitive interests to both esoteric and wider social interests, it will be possible to provide a way of answering the question posed at the beginning of this section as to the relation of science and society. For if it can be shown that a particular wider social interest sustained a given cognitive interest and that the development of a scientific theory reflected that cognitive interest, then an effect of the wider society on the conceptual development of the theory can be said to have been exhibited. If such a connection does not exist, and social interests internal to a scientific specialty, such as in extending the range of problems solvable by its practitioners, adequately account for those cognitive interests that are present, it can be claimed that the relative autonomy of the scientific field in question has been demonstrated. Alternatively, both kinds of social interests may be operating, and may interact/...

act to reinforce or crosscut each other, and an account that is neither simply 'internalist' nor simply 'externalist' will be produced.

1.4 A Preliminary Remark on the Sociology of Knowledge

The question of social interests and their relationship to knowledge takes us into the traditional sphere of the sociology of knowledge. A lengthy discussion of this would be quite out of place, but in order to prevent possible misunderstanding of the nature of some of the accounts put forward in subsequent chapters, one preliminary point needs to be made.⁽¹⁵⁾ That is that no attempt is being made here to provide a causal explanation of the thought of particular individuals. Such an account might, in principle, be possible, but the lack of sufficiently strong psychological and sociological theories and the sparseness of evidence certainly render it impracticable. Thus, we might try to explain an individual's thought as follows: Individual X holds belief B. X is a member of Group G. All members of G believe B. The criteria for membership of G are 'objective'; it is not a group self-selected on the basis of attachment to B. B is a belief 'appropriate' to G in its social situation. This format, it would appear, is that which many historians and sociologists of science think/...

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(15) Below, this point is made in terms of social explanations involving social groups such as classes. Identical considerations apply also to explanations in terms of more limited groups, such as the practitioners of a particular scientific discipline or specialty.

think of when discussing explanation in the sociology of knowledge. It is obviously unrealisable in practice. Groups are not homogeneous in their beliefs, and, if we weaken 'all members of G believe B' to 'most members of G believe B', then the explanation of X's beliefs is greatly weakened. We often find that groups, or large sections thereof, hold beliefs that seem inappropriate to their situations. Members of subaltern groups frequently subscribe to those beliefs that are used to defend the system of domination under which they labour. Further, nearly all groups are to some extent 'self-selected'. Individuals may choose to give their allegiance to, or to seek membership of, groups quite different from those into which they were born. Ideologies deemed appropriate to one group are on occasion developed by individuals who by birth or occupation are in fact members of quite a different group. (16)

The difficulties of the sociology of knowledge so conceived need not, however, force us to abandon a sociological perspective. There is an alternative approach that eschews the attempt to explain in a strong sense the thought of particular individuals, but which instead relates thought to social structure in roughly the following manner. First of all, we have to identify social positions whose occupants may reasonably be held to have/...

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(16) See Child (1941; 1944) and Barnes (1977) for a discussion of these problems of 'imputation'.

have similar interests and experiences. We then argue that these interests and experiences constrain the set of beliefs 'appropriate' to occupants of these positions. 'Appropriate' beliefs will be ones justifying a group's privileges, advocating an advance in its situation, furthering its coherence or the interests of its members and reflecting the salient features of the typical experiences of its members. It is not that the nature of an appropriate 'class consciousness' can be deduced a priori from the position of the group within the social structure, for the pre-existing states of belief and the ideologies of other groups obviously affect the beliefs appropriate to the group. Nor is there any reason why only one set of beliefs should be appropriate to a group; indeed, conflicting beliefs may arise, reflecting, for example, different aspects of its experience, or tensions between the short-term and long-term interests of its members. These provisos aside, it should be possible to identify 'tendencies' of thought that express the influence of the social situation of the group. These need not be manifest in the thought of all of the group's members, nor even in that of a majority of them. Nor need they be restricted in their manifestation to the members of the group: outsiders who identify with the group may well manifest them, often, indeed, in heightened form. (17)

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(17) The above approach to the sociology of knowledge follows, at least in its general thrust, that of Lukács (1971) and Goldmann (1964).

An analogy from the sociology of politics may clarify the status of this kind of explanation. To say that political party P expresses the interests of group G is not to imply that all members, or even most members, of G vote for P. It is rather to assert that P's policies, if put into effect, would enhance the wealth, status, power, security and so on of G. Differential support for P between members and non-members of G might then be anticipated, but the point is that the core of the argument is structural and not individual.

The virtue of this approach to the sociology of knowledge is that it does not attempt the practically impossible task of giving deterministic accounts of particular individuals, and that it takes into account the complexities of group affiliation. Obviously, it is a theoretical rather than an empirical approach, requiring a theoretical account of social structure and of the interests associated with the different locations in that structure. A case study such as this cannot claim in any sense to prove the correctness of such an approach. To attempt such a proof would take us of necessity into a study of the social relations of belief much wider than that undertaken here. My aim is more modest: to construct some tentative hypotheses about the relationship between belief and social structure in the period discussed, and then to argue that these hypotheses at least make sense of the historical materials presented here.

1.5/...

1.5 Outline of Subsequent Chapters.

As the preceding two sections have indicated, I shall be attempting to examine in detail some particular developments, and to suggest some general explanatory hypotheses. In the following chapter, one particular individual will be considered: Francis Galton. Galton is generally acknowledged as a founder of modern statistics (Cowan, 1968). But Galton was also the founder of the eugenics movement, and it will be argued in chapter two that it was his eugenic concerns which led him to his breakthrough in statistical theory. In chapter three, the focus will therefore broaden to consider the British eugenics movement, and an attempt will be made to relate the beliefs and fortunes of this movement to aspects of the changing British social structure. The general perspective developed in this chapter will be applied in chapter four to Karl Pearson, the crucial figure in the continuance of the line of work begun by Galton, and a man who laid many of the intellectual and institutional foundations of modern statistical theory. The relationship between Pearson's social position and his beliefs will be discussed, as will that between the political and philosophical aspects of his beliefs and his science. Chapter five will then examine the impact of Galton and Pearson, and their role in creating the nucleus in Britain of statistical theory as a scientific specialty.

When/...

When beliefs go unchallenged, it is sometimes difficult to discern which aspects are primary and which secondary, which their holders are prepared to discard and which are central. Scientific controversy, the clash of differing theories, is therefore particularly important to the historian in throwing light on the nature of the systems of belief being studied. In chapters six and seven, two crucial controversies involving Karl Pearson and his followers will be examined. The first is the famous and bitter dispute between Pearson and the early Mendelian geneticists. Although this dispute takes us outside the boundaries of statistical theory, it allows a broader view of the scientific programme of Karl Pearson and his followers. Further, it provides an interesting testing ground for the framework of explanation developed in the foregoing chapters. The second controversy is much less well known. It was the first major dispute to divide the emerging community of mathematical statisticians, setting Pearson and those loyal to him against his former pupil George Udny Yule. Again, an attempt will be made to demonstrate the usefulness of the overall perspective of this thesis in understanding this rather esoteric disagreement.

The last substantive chapter points forward from the zenith of the influence of Francis Galton and Karl Pearson. Neither Galton nor Pearson devoted much attention to the knotty problems of the general methodology of/...

of statistical inference. They were primarily concerned with developing substantive techniques and theories adequate for the development of their chosen field of research. In this chapter, Karl Pearson's somewhat informal views on statistical inference will be reconstructed. The work of Gosset and Fisher in transforming the Pearsonian approach to inference will then be presented, and it will be suggested that two new developments can be detected in their work. The first is the beginning of the systematic application of statistical methods in agriculture and industry. The second is the beginning of what we might term 'meta-statistics': the attempt to systematise statistical techniques developed in response to particular problems, and to forge a consistent theory of statistical inference.

The conclusion will return to some of the issues raised in this introductory chapter concerning the influence of social interests on science, and of the problems of applying the sociology of knowledge to science. The implications of this case study for these issues will be examined.

Chapter Two

Francis Galton and Statistical Theory

With his concepts of regression and correlation, Francis Galton achieved a major breakthrough in statistical theory. He did not simply add to a stockpile of available technical tools, but opened up a whole new area of application and theoretical development. His work represents the natural starting point for a history of the development of modern statistical theory in Britain. However, in order to understand the radical nature of his innovations, it is necessary first to sketch in something of the background against which he must be placed.

2.1 Statistics, Probability and Error

There were three main traditions of work from which mathematical statistics in Britain might plausibly have sprung. The first of these was the work of the unofficial and official gatherers of statistical information, in particular of the Statistical Societies. The second was work by mathematicians in the theory of probability, and in its actuarial applications. The third was the work of mathematical physicists and astronomers, most notably in the field of error theory, but also in the kinetic theory of gases.

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In terms of numbers of researchers and volume of work produced, official statistics and empirical social research together form by far the most important of these three areas. Early Victorian Britain saw a 'statistical movement' which, although relatively short-lived, gave birth to a tradition of empirical social research and contributed much to the development of official statistical agencies.⁽¹⁾ This movement, and the Statistical Societies it gave rise to, were by no means committed to a sophisticated mathematical methodology. The term 'statistics' had, originally, no such connotation. The 1797 edition of the Encyclopaedia Britannica defined the term as a 'word lately introduced to express a view or survey of any kingdom, county, or parish' (Cullen, 1975, 10-11). It was only very gradually that 'statistics' came to refer exclusively to quantitative studies. 'The contents of the Journal of the Royal Statistical Society would suggest that it was not until the present century that "statistics" came to mean solely numbers and the methods of analysing numbers' (Cullen, 1975, 11). The early Victorian statistical movement should thus be seen not as the forerunner of the modern discipline of statistics/...

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- (1) The main source here is Cullen (1975), but see also Abrams (1968), Elesh (1972), Cole (1972) and G.M. Young (1960, especially 32-3). More generally, see Oberschall (ed.) (1972), Stephan (1948), Lazarsfeld (1961), Lécuyer and Oberschall (1968). Baines (1970) describes the general features of the nineteenth century British system of official statistics, while Royal Statistical Society (1934) describes the development of that Society. For medical statistics see Greenwood (1936; 1948), Hodgkinson (1968), Hilts (1970) and Eyler (1973; 1976).

statistics, but, Cullen argues, as a group of social reformers producing and utilising 'facts' to advance their programmes.

The statisticians wanted to contribute more than voluntary and legislative action in the fields of public health and education: they were also free traders, supporters of the new poor law (if not framers and administrators of it), opposed to trade unions and working class radicals, suspicious of factory acts.
(Cullen, 1975, 147)

The squalor and unrest of early Victorian Britain was portrayed by the statistical movement as the consequence not of industrialisation nor of capitalism, but of urbanisation and its harmful effects on the physical and moral well-being of the working class. The remedy then lay in reforming that urban environment, chiefly through education and public health measures, rather than in more radical change.

The most permanent institutional legacy of the statistical movement was the London Statistical Society, founded in 1834, which in 1887 became the Royal Statistical Society. This body reflected the concerns of the movement from which it sprang.

Although there were a few mathematicians among the original members, there were many more economists, politicians, peers, government officials, and doctors of medicine: their object was politically useful information about society, not, say, the development of mathematical method.
(Abrams, 1968, 14)

In the first fifty years of its existence, only 11 out of the 511 papers read to the Society concerned statistical method (Abrams, 1968, 16). Towards the end of the nineteenth/...

nineteenth century a somewhat greater interest in method can be seen in its Journal, but this was largely imported from outside, as mathematical statisticians such as George Udny Yule began to use the techniques they had learned elsewhere to illuminate some of the traditional problem areas of the Statistical Society (see below, chapter seven). Even this development was extremely slow. In the 25 years between 1909 and 1934, only 15 out of 212 papers dealt with the 'collection and methods of statistics' (Royal Statistical Society, 1934, 205). In the period discussed in this thesis, the Royal Statistical Society, for all its importance and governmental influence, was largely irrelevant to the development of statistical theory. Its interests lay elsewhere. (2)

The tradition of empirical social research that sprang from the statistical movement was also largely barren of methodological advances. As Stephen Cole argues, there was a lack of intellectual continuity, presumably resulting from the amateur and non-institutionalised nature of this field of study.

It is very rare that any one of the researchers would pay enough attention to past work to either criticise it ... or to make advances on it. An intellectual history of British social research in the nineteenth century would not be very interesting reading. The end would be no different than the/...

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- (2) Economic statistics was perhaps the area of activity of the Society and related bodies which was most highly developed. In the period discussed in this thesis, F.Y. Edgeworth and A.L. Bowley (see chapter five) were leading experts in this field.

the beginning and the middle no different than either end.

(Cole, 1972, 108)

It was not until well into the twentieth century that we find anything other than the most elementary use of statistical techniques. Systematic or random sample survey methods, for example, were not used in Britain until 1912 (Stephan, 1948, 22; Bowley and Burnett-Hurst, 1915).

Earlier surveys had either aimed at complete coverage, or had used specially selected subgroups of the population. (3)

Sophisticated designs or complex quantitative analyses were not to be found. At best, investigators sought adequate and representative coverage, and calculated those summary statistics they felt to be of substantive importance.

After all, they were, in general, reformers concerned with influencing public policy and convincing lay audiences.

A display of technical expertise beyond a basic minimum might well have been counter-productive, even had the researchers had the competence and motivation to engage in it.

Medical statistics was developed to a somewhat greater/...

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- (3) Thus Booth, in his famous survey of London, chose to examine families with school children, and to argue that the condition of the entire population would be the same 'or so far as there is any difference better rather than worse' (quoted by Cole, 1972, 81).

E. Yeo (1976) has made the interesting suggestion that sampling was not used in early British studies of poverty because the complete door-to-door survey was valued in itself as a form of contact between 'rich' and 'poor'.

greater level of sophistication than social statistics. The 1836 Registration Act established a national system for the recording of births, marriages and deaths, and a standardised system for the reporting of causes of death was introduced by William Farr. Death returns of large towns were published in the newspapers, and infant mortality rates and mortality rates for specific diseases were calculated. This data was put to considerable use, both administratively and for research into the causes of disease. But again the level of sophistication thought necessary or desirable in the analysis of this data was low.

Mathematical precision might have helped, but few of our modern techniques existed and where they did were little used. Not even Farr was a good mathematician. People worked with figures using totals and bare averages. Producing the raw materials was sufficient to obtain results. (Hodgkinson, 1968, 185)

Methodological debate there certainly was, but it concerned the inadequacies of the reporting system and gross errors, such as the comparative use of crude mortality rates without taking into consideration the age composition of populations (Eyler, 1976). The immediate problems faced by those producing and using vital statistics were not such as to suggest that the search for sophisticated analytic tools was the way forward.

Turning to probability theory, we find an area of much less activity than that generated by the 'statistical movement'. If we leave aside the rather sporadic controversy, which was of a philosophical rather than mathematical/...

mathematical nature, about the definition of probability (this is returned to briefly in chapter eight), the theory of probability was something of a backwater in nineteenth century Britain.⁽⁴⁾ The work of the Continental probability theorists, especially P.S. de Laplace (1814), overshadowed British contributions. Todhunter's compendium of the theory of probability, perhaps itself the most important British contribution to the field, ended with the work of Laplace (Todhunter, 1865). Little significant original research was done. Kendall and Doig's bibliography (1968) reveals only sporadic and on the whole relatively trivial work on probability theory by British mathematicians. Of course, that is not to deny that acquaintance with the field was widespread. The theory of probability had become an established part of the mathematical curriculum. Textbooks of algebra, even at a relatively elementary level, contained rules for and exercises in the manipulation of probabilities (see, for example, Todhunter, 1858). The student of mathematics would have been exposed from an early stage to such topics as the addition theorem for the probabilities of mutually exclusive events, and the multiplication theorem for the probabilities of independent events. Simple combinatorial algebra, together with these elementary theorems, would, however, have seen the student through most of the problems in/...

(4) The three major recent works in the history of probability theory are David (1969), Maistrov (1974) and Hacking (1975). None of them, however, deals with the period discussed here.

in probability theory he was likely to meet, even at the relatively advanced level of the Cambridge Mathematical Tripos, as is revealed by the examination questions in Gantillon (ed.)(1852). In sum, probability theory was an established field of accepted knowledge, but not a prime area of ongoing research. Perhaps an explanation for this can be found in Boyer's suggestion (1968, 621) that British mathematical research after the Napoleonic Wars was focused in those areas where Continental achievement had been least. A British mathematician wishing to make his reputation might well have thought it advisable to avoid devoting a major research effort to a field which had been cultivated in so magisterial a fashion by an acknowledged giant such as Laplace.

Actuarial work, where probability theory was linked to the data collected by vital statisticians in an enterprise of commercial importance, provided at least temporary employment for several important British mathematicians in the nineteenth century. Actuarial science

... consists of the application of the laws of probability to insurance, and especially to life insurance. And we shall find that, first of all, there needed to be something for the law of probability to act upon, viz., a mortality table, and also a handmaiden, in the form of a developed law of compound interest and discount and of annuities certain.
(Dawson, 1914, 95)

As Dawson's review shows, the nineteenth century saw many advances in actuarial science. It would seem, however, that/...

that by this period the major technical instruments of actuarial work, such as the life table, had been developed. There was, of course, much theoretical and empirical work to be done to improve them, but this had become a fairly specialised line of work. Actuarial work was thus rather insulated from developments in statistical theory generally during this period.⁽⁵⁾

Error theory was in a broadly similar condition to the theory of probability.⁽⁶⁾ Like the theory of probability, error theory had been developed most fully by Continental workers, most importantly Gauss. It had become an important and established field, systematically presented in textbooks, of which the most widely used seem to have been Airy (1861) and Merriman (1901; first published in 1884). Here was to be found the intellectual resources which statisticians like Galton and Pearson were first to turn to, such as the 'error curve' or 'law of frequency of errors' and the 'probable error'. Walker (1929, 50) explains these/...

(5) One leading British actuary, William Palin Elderton, played a subsidiary role in the development of the biometric school. See chapter five.

(6) For the history of error theory see Merriman's annotated bibliography (Merriman, 1877). More recent historical work, not, however, covering this period, includes Tilling (1973), Sheynin (1966; 1971a; 1972b; 1973a), Plackett (1972). Seal (1967) is the only recent work to cover the development of the theory of errors after Gauss at all extensively.

these chief theoretical concepts of the error theorists, and from her description we can see how these concepts related to the cognitive interests discussed in section 1.3:

The term probable error originated among the German mathematical astronomers who wrote near the beginning of the nineteenth century. The early use of the term is in certain memoirs dealing with astronomy, geodesy, or artillery fire, where the writer is attempting to make the best possible determination of the true position of a point from a series of observations all of which involve an element of error. A deviation from the true position of the point, or more commonly from the mean of the observations, of such a magnitude that, if the number of observations be indefinitely increased, one half of the errors may be expected to be numerically greater and one half numerically less than this value, is then termed the 'probable error'. When the frequencies of the various errors are plotted, the result is quite naturally spoken of as the 'curve of facility of error', or 'curve of error', and the formula describing it as the 'law of facility of error', 'law of error', and 'error function'.

It was generally assumed during the nineteenth century that what we now call the 'normal' or 'Gaussian' distribution was the law of error: it was, for example, sometimes referred to simply as the 'probability curve'.

Despite, or perhaps because of, its accepted and established status, error theory was not an important area of theoretical work by British mathematicians. Only 14% of the 408 books and memoirs in the field surveyed by Merriman (1877) were published in Britain. The most consistent work was that of the Cambridge astronomer and mathematician, J.W.L. Glaisher (for whom see Forsyth, 1929). Even in his case the theory of errors was only a subsidiary interest/...

interest, and his major paper in the area (Glaisher, 1872), while a thorough and critical review of the literature, did not constitute a crucial methodological advance.

Galton's work aside, there was little attempt in Britain to develop Quetelet's insight that the error theorists' 'probability curve' could be applied to social as well as physical phenomena. Indeed, Gillispie (1963) suggests that the impact of Quetelet's work was, paradoxically, most immediate in British physics (though see also the comments by Hesse, 1963). In any case, it seems established that James Clerk Maxwell read the review of Quetelet (1849) by Herschel (1850), and may have been encouraged by the example of Quetelet's work to use the theory of probability to construct models of physical phenomena: that is, to develop a statistical mechanics.⁽⁷⁾ Statistical mechanics became an important area of study for British mathematical physicists in the later part of the nineteenth century. This initial episode apart, statistical mechanics and statistical theory proper seem, however, to have developed more or less independently in the period discussed here.⁽⁸⁾ Perhaps this was because statistical theorists were attempting to devise tools for drawing inferences from data, while the physicists were attempting to construct deductive models that would explain/...

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(7) See Brush (1967, 152) and Garber (1973, 19-29).

(8) One exception is the attempt by Burbury (1894; 1895; 1899) to take up Galton's work on correlation and apply it in statistical mechanics. See appendix C.

explain the observed behaviour of gases. Whatever the explanation, British statisticians seem to have found few intellectual resources to exploit within statistical mechanics.

The conclusion that has to be drawn from this brief survey of social statistics, probability theory, error theory and statistical mechanics is that none of these fields gave rise, in nineteenth century Britain, to any substantial and significant contributions to or interest in the mathematical theory of statistics.⁽⁹⁾ One consequence of this is that Francis Galton, although a scientist of considerable status and with wide-ranging contacts in the scientific community, seems for much of the period of his work in statistics to be ploughing a rather lonely furrow. Although he had many admirers, and obtained the temporary assistance of several mathematicians, it was not until quite late in his life that he found followers to develop his innovatory work. It is to this innovatory work that we must now turn.

2.2 Galton's Early Work in Statistical Theory

Galton's/...

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- (9) The question 'Why were these areas not productive of statistical theory?' is somewhat misleading, in that workers in these areas did not see themselves as contributing to an entity labelled 'statistical theory', but were engaged in the pursuit of goals of a different and narrower nature. Quite aside from the general difficulties of trying to explain the reasons why something did not happen, a fuller study of the issues raised by this question would take us too far away from the main subject of this thesis.

Galton's main contributions to statistical theory were his pioneering of rank-ordering methods (the notion of the median, quartiles, etc.) and, more importantly, his invention of the concepts of regression and correlation. To describe these innovations in detail is unnecessary: there is a full account in Karl Pearson (1914-30). Instead, I propose to examine the impetus behind Galton's statistical work, and the effect of that impetus on the content of his work. (10)

That impetus was eugenics. This has been shown most clearly by Ruth Schwartz Cowan (1972a), who concludes (527-8):

Galton created biostatistics while he was in pursuit of a solution to the problem of heredity. He dreamed of a truly eugenic society, a society based upon the laws of heredity: the laws of heredity would guide the breeding habits of men, and the evolutionary welfare of the race would become a moral criterion ... [Galton's] eugenic dreams had provided him with the motivation and the mental perseverance that he needed to unlock the secrets of probability.

It is, however, clear also from Karl Pearson's biography (1914-30, 3A, 434-5):

There was a unity underlying all Galton's varied work ... which only reveals itself when, after much inquiry and retrospection, we view it as a whole and with a spirit trained to his modes of thought ... From 1864 to 1911 Galton achieved in many fields, yet in 1864 he had realised his life-aim - to study racial mass-changes in man with the view of controlling the evolution of man, as man controls that of many living forms.

Initially/...

- (10) The main biographical sources for Galton are K. Pearson (1914-30) and Forrest (1974). For Galton's statistics see also Cowan (1972a) and Hilts (1973).

Initially, Galton simply used existing statistical concepts in eugenic theorising. His first paper on eugenics used only elementary quantitative arguments to bolster its conclusion that human mental characteristics were inherited and that 'the improvement of the breed of mankind is no insuperable difficulty' (Galton, 1865, 319-20). His first major work on eugenics, Hereditary Genius (1869), went further than this by following Quetelet in applying the 'law of error' to human populations.⁽¹¹⁾ Galton argued, though could not prove, that the law applied to mental characteristics, as well as the physical ones discussed by Quetelet:

This is what I am driving at - that analogy clearly shows there must be a fairly constant average mental capacity in the inhabitants of the British Isles, and that deviations from that average - upwards towards genius, and downwards towards stupidity - must follow the law that governs deviations from all true averages.
(Galton, 1869, 32)

Galton soon began to find that he needed tools different from those provided from the error theorists.⁽¹²⁾ For the latter, variability ('error') was something at best to be eliminated, or at worst to be controlled and measured. The cognitive interests manifested in error theory thus militated/...

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(11) Galton learnt of the law of error from his friend William Spottiswoode, the geographer. See Galton (1908, 304), also Cowan (1972a, 512) and Spottiswoode (1861).

(12) The argument of this paragraph is largely that of Hilts (1973).

militated against the treatment of variability as a phenomenon in its own right. For Galton, as a eugenicist, human variability was the potential source of racial progress. Galton's eugenics thus led in his statistical work to an orientation towards variability as an intrinsically important phenomenon.⁽¹³⁾ In the light of this orientation, error theory concepts could be judged to be restrictive and misleading, even absurd (Galton, 1875a, 35).

Galton's break with the error theory approach to variability is best seen in his paper 'Statistics by Inter-comparison' (1875a). In this paper he sought replacements for the error theory measures of central tendency (the mean) and of variability (the probable error). He did this by the use of relative rank rather than absolute value as the basis of his statistical analysis. He later justified this approach by explicit reference to the characteristics of social life (1889c, 474):

Relative rank is, however, on the whole, a more important consideration than the absolute amount of performance by which that rank is obtained. It has an importance of its own, because the conditions of life are those of continual competition, in which the man who is relatively strong will always achieve success, while the relatively weak will fail. The absolute difference between their powers matters little.

Galton/...

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- (13) It could be said that the work of both Galton and the error theorists manifested a cognitive interest in the prediction and control of variability, but this interest took different particular forms, the error theorists seeking to minimise the effects of 'error', while Galton sought to preserve and make use of (biological) variability.

Galton would rank-order a set of individuals or objects by comparing them one against the other according to some quality.

The object then found to occupy the middle position of the series must possess the quality in such a degree that the number of objects in the series that have more of it is equal to that of those that have less of it. In other words, it represents the mean value of the series in at least one of the many senses in which that term may be used. (1875a, 34; Galton's emphasis)

This value Galton was later (1883, 52) to term the 'median value'.⁽¹⁴⁾ To measure variability Galton used what were later called the quartiles: those objects such that one quarter and three quarters of the objects had smaller values of the quality in question. Half the inter-quartile distance was then a useful measure of the variability of the objects. Although Galton generally continued in his published work to use the terms 'mean' and 'probable error', in his actual calculations the median and inter-quartile distance are more frequent (see, for example, Galton 1888b).⁽¹⁵⁾

Galton's negative evaluation of error theory and his introduction of rank-ordering methods in statistics can therefore/...

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(14) Fechner (1874) independently developed the concept of the median value, der Centralwerth. (I owe the reference to Walker(1929, 184).)

(15) Of course, for a normally distributed population the median and half the inter-quartile distance are equal to the mean and probable error respectively. Galton presumably continued to use the old terms so as to be understood by those trained in error theory.

therefore be traced to the operation in his case of cognitive interests different from those to be found in error theory.⁽¹⁶⁾ They can also be taken as indicators of a new approach to the statistics of distributions. Even though his followers such as Pearson preferred, for reasons of mathematical tractability, to use the earlier formulae (mean instead of median, etc.), this shift of focus was to continue. Thus there was a gradual transition from use of the term 'probable error' to the term 'standard deviation' (which is free of the implication that a deviation is in any sense an error), and from the term 'law of error' to the term 'normal distribution'.⁽¹⁷⁾

Galton himself became aware of the divergence between his approach and that of the error theorists, and of the reasons for it. He wrote in his autobiography that some of his applications of the 'Gaussian Law' seemed 'to be comprehended with difficulty by mathematicians'. He went on to explain this in terms of what have here been called different 'cognitive interests':

The primary objects of the Gaussian Law of Error were exactly opposed, in one sense, to those to which I applied them [sic]. They were to get rid of, or to provide a just allowance for errors. But these errors or deviations were the very things I wanted to preserve and to know about.
(1908, 305)

Galton's/...

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(16) Alternatively, we could talk of the same general cognitive interest differently 'particularised'.

(17) For the history of these terms, see Walker (1929, 185 and 188).

Galton's work represents, however, much more than a shift in general focus in statistical theory. The error theorists had worked predominantly with distributions of one variable or, at most, of mutually independent variables. (18) Galton provided, in the concepts of regression and correlation, the key tools for the treatment of two dependent variables, and made the advance to the general treatment of any number of dependent variables a relatively easy technical problem. His work in this area arose directly from his eugenic concerns.

In the last chapter of Hereditary Genius, Galton discussed the relationship between parent and offspring generations. He envisaged the development of a predictive, quantitative theory of descent. It might be possible, for example, to deduce the average contribution of each ancestor to the hereditary make-up of a child:

Suppose, for the sake merely of a very simple numerical example, that a child acquired one-tenth of his nature from individual variation, and inherited the remaining nine-tenths from his parents. It follows, that his two parents would have handed down only nine-tenths of nine-tenths, or $81/100$ from his grandparents, $729/1000$ from his great-grandparents, and so on; the numerator of the fraction increasing in each successive step less rapidly than the denominator, until we arrive at a vanishing value of the fraction.
(1869, 371)

At first Galton felt that this theory could be developed/...

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(18) There are exceptions to this generalisation.
See section 2.4.



developed from physiological considerations, in particular Charles Darwin's 'provisional hypothesis of pangenesis' (Darwin, 1868, 2, 357-404, especially 374). Pangenesis, however, proved to be a blind alley. Galton attempted to test the theory by transfusion experiments in rabbits. If the blood did indeed contain 'gemmules' which aggregated into the sperm or ova, as the theory of pangenesis appeared to suggest, then the offspring of a rabbit that had received a massive transfusion of the blood of another rabbit should show a tendency to resemble the latter rather than the former. Galton was unable to find any such effect, and concluded that the theory was untrue.⁽¹⁹⁾ He put forward his own corrected version of pangenesis, without freely circulating gemmules (Galton, 1875b). This alternative theory had the additional feature of effectively denying the possibility of the inheritance of acquired characteristics, a possibility which had been affirmed in Darwin's original theory of pangenesis and which Galton had apparently never liked.⁽²⁰⁾ While Galton's new theory made possible what is arguably the first clear statement of what is now called the genotype/phenotype distinction (Galton, 1875b/...

(19) K. Pearson (1914-30, 2, 156-66); Cowan (1977, 173-9). Darwin responded to Galton's experiments by claiming that it was not an essential part of the theory of pangenesis that the 'gemmules' in fact circulated in the blood and that they could travel through the body in other ways.

(20) For Galton's attitude to the theory of the inheritance of acquired characteristics, see Cowan (1968; 1977).

1875b, 94), it did not lead to the development of a mathematical law connecting parent and offspring generations. To find this, Galton had to turn to direct experiment.

Ideally, Galton would have preferred to use human data; however, these were as yet unavailable, and he turned to a more convenient alternative. He began work on sweetpea seeds, though

It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man.
(Galton, 1885a, 507; quoted by Cowan, 1972a, 517)

His scheme was to grow sweetpeas from seeds of a measured size, and then to measure the seeds produced by these plants. The second generation of seeds could then be considered the offspring of the first. Galton would thus have the data for a direct numerical examination of the relationship between two generations connected by heredity. He began the experiment by taking several thousand sweetpea seeds and weighing them individually, thus obtaining the mean and probable error of the distribution of weight. He then made up several sets of seeds. Each set consisted of seven packets, each packet containing ten seeds of exactly the same weight. The weights were chosen so that one packet contained very small seeds (with weights given by the population mean minus three times the probable error), the next slightly larger (weight equal to the population mean minus twice the probable error) and so on up to a packet with giant seeds (weight equal to the population mean plus three times the probable error). Nine of these sets/...

sets were made up, and Galton sent them to friends to grow. Two sets failed, but he obtained the produce of the other seven sets.

He presented the results of the experiment in a lecture delivered at the Royal Institution on 9 February 1877 (Galton, 1877; the data on which the statements in this lecture were based were never fully published).⁽²¹⁾ Galton said that an exceedingly simple law connected parent and offspring seeds. Let the mean of the parent generation be M and its probable error be Q . Then the parent seeds fall into the seven categories $M - 3Q$, $M - 2Q$, $M - Q$, M , $M + Q$, $M + 2Q$, $M + 3Q$. The offspring of each category of parent had weights distributed according to the law of frequency of error, and the probable error of each group of offspring was the same: the offspring of the smallest seeds were no less variable than the offspring of the largest seeds. But the mean weight of each class of offspring was less extreme than that of their parents. As Galton put it, 'reversion' had taken place. Further, this reversion was linear. That is, the seven parent categories gave rise to seven offspring classes with means $M - 3bQ$, $M - 2bQ$, $M - bQ$, M , $M + bQ$, $M + 2bQ$, $M + 3bQ$, where b is a positive constant less than one.⁽²²⁾

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(21) Galton (1885c, 258-60) discussed the relationship between the diameters of parent and offspring seeds and gave a table of figures for this. The original records of the sweetpea experiment have not been found in the Galton papers.

(22) Galton used 'r' not 'b'. I have changed his notation to make it clear that, to use Galton's later terminology, the constant is a coefficient of regression, not of correlation.

A hypothetical example may make this clearer. Suppose the parent generation to have mean 100 units and probable error 20 units. Then we have seven sets of seeds of weights 40, 60, 80, 100, 120, 140, 160 units. The mean weight of the offspring of the parent seeds weighing 40 units is not 40 units but 70 units; the offspring of the parents weighing 60 units have mean weight 80 units; of those weighing 80 units, 90; of those weighing 100, 100; of those weighing 120, 110; of those weighing 140, 120; of those weighing 160, 130. In this case $b = \frac{1}{2}$; the offspring seeds differ from the mean by only half as much as their parents.

On the face of it, this is an odd result. Does it not mean that the offspring generation will be clustered round the mean much more closely than the parent generation? Galton was, of course, well aware that the curve representing the distribution of a particular character in a species ordinarily remains virtually identical from one generation to another (Galton, 1869, 27). He argued that linear reversion to the mean was in fact part of the process by which the stability of the distribution was maintained from generation to generation. The 'compression' of the distribution due to reversion would be balanced by the 'expansion' due to fraternal variability (that is, to the variability within groups of brothers and sisters). Galton suggested that this process could be seen as having, in theory, two parts. We start with a parent generation with probable/...

probable error c_1 . Reversion we imagine as 'compressing' this distribution to one with a probable error of bc_1 (b is less than one). We now imagine each parent as tending to breed true to the (reverted) parental type, but the offspring of each parentage having a probable error f . The 'error' of the offspring generation (c_2) will thus be the resultant of the two independent 'errors' bc_1 and f :

$$c_2^2 = b^2 c_1^2 + f^2$$

Parent and offspring generations can then have equal variability ($c_1 = c_2$) provided $f^2 = (1 - b^2) c_1^2$: this is a quantitative statement of the balancing of fraternal variability and the reduction in variability due to reversion.

Unlike sweetpea seeds, human offspring have more than one parent. Galton found a neat device for handling this problem: the mid-parent. The mid-parent was a fictitious amalgam of the characteristics of father and mother. Thus the mid-parental height was the mean of the paternal and maternal heights, with female height adjusted to allow for the greater mean and probable error of male height. Offspring could now be considered as descended by uniparental inheritance from this mid-parent. The population of mid-parents has, because of its construction, a smaller variability than either paternal or maternal populations. To see why this is so, let paternal height be x , let maternal height (adjusted to make it comparable with paternal height) be y , and let the probable error of the paternal generation (which is equal to the probable error/...

error of the adjusted maternal generation) be c . The formula for mid-parental height is simply $\frac{1}{2} (x + y)$. So the square of the probable error of the mid-parental population is given by $\frac{1}{4} (c^2 + c^2)$, if we can assume paternal and maternal height to be independent (that is, no assortative mating). This gives a probable error for the mid-parental population of $\frac{c}{\sqrt{2}}$, not c . As we shall see, this difference was to be of some importance in the development of Galton's later work.

In retrospect, this paper (Galton, 1877) can be seen as the first stage of Galton's revolution in statistical theory: his first development of the concept that was later to be called linear regression. However, Galton did not at the time see himself as doing anything other than contributing to knowledge about heredity, as is indicated by his use of the ordinary biological term 'reversion'. Further, it may appear from the account so far given that the 'law of reversion' was reached purely empirically; that Galton simply looked at the data and deduced the law. This is most unlikely. Galton had a definite prior notion of the kind of law he was looking for: a simple, predictive, mathematical statement of the relationship between parent and offspring generations. There is indeed reason to believe that his data did not unequivocally 'suggest' the law of reversion. Some later comments by Galton indicate this. Thus he wrote (1885c, 259):

I possessed less evidence than I desired to prove/...

prove the bettering of the produce of very small seeds.

His data was not even sufficiently good to enable him to give a numerical value for the coefficient of reversion:

The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it.
(1885a, 507)

It would therefore seem that Galton was seeking to show order in his data, rather than the data spontaneously manifesting order.⁽²³⁾

2.3 The Bivariate Normal Surface and Correlation

By the end of the 1870's Galton had thus broken with the error theory approach to statistics, even though he still used the rather incongruous error theory terminology. He had also made the first decisive step, with his law of reversion, in developing a statistical theory of two dependent variables. The 1880's saw him consolidate this early work, develop the theory of the bivariate normal distribution, and move from the concept of reversion to that of correlation.

In the early 1880's Galton began to seek anthropometric data of direct relevance to problems of human heredity. This/...

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(23) K. Pearson (1914-30, 3A, 4) has reconstructed from Galton's seed diameter data a diagram which shows the relatively poor approximation of this data to the simple 'law of reversion'.

This data could, he felt, be of other than purely scientific use. As the notion that human characteristics were predominantly hereditary became more and more established, it was 'highly desirable to give more attention than has been customary hitherto to investigate and define the capacities of each individual' (1882, 333). With this information, Galton felt that a better fit of individuals and their social roles could be achieved. Galton called for the establishment of 'anthropometric laboratories' in which individuals and whole families could have a wide range of physical and mental traits examined and measured.

In 1884 Galton set up just such a laboratory at the International Health Exhibition held in South Kensington. By 1885 over 9,000 people had paid the small fee and been measured for keenness of sight, colour sense, 'judgment of eye', hearing, highest audible note, breathing power, strength of pull and squeeze, swiftness of blow, span of arms, height standing and sitting, and weight (Galton, 1885b). The offer of public prizes for the best-kept 'family records' brought in another body of important anthropometric data (Forrest, 1974, 179-80).

With this data Galton was able, in effect, to repeat the sweetpea study on human beings. He revealed his first results in his Presidential Address to the Anthropological Section of the British Association (1885a). (Further details were published in his (1885c) and (1886)). On/...

On the basis of the family records already obtained he claimed:

An analysis of the records fully confirms and goes far beyond the conclusions I obtained from the seeds.
(1885a, 507).

The particular human trait he chose to investigate was stature. It was easy to measure, relatively constant during adult life, its distribution closely followed the law of frequency of error, and assortative mating according to stature was, Galton argued, negligible. Galton had to hand the stature measurements of 928 adult children and of their 205 parentages. For each parentage he calculated the height of the mid-parent by multiplying the mother's height by 1.08 and taking the mean of that and the father's height. He was then able to investigate the relationship of offspring height to mid-parental height.

Galton found that this human data showed clearly the pattern more ambiguously manifested by the sweetpea data. A relationship of linear reversion (or 'regression' as he now called it)⁽²⁴⁾ existed between offspring and mid-parental heights. A mid-parental deviation of one unit implied an expected offspring deviation of $2/3$ of a unit ($b = 2/3$), and the probable error of the offspring of each class of mid-parent was constant.

Galton/...

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(24) Cowan (1972a, 520) argues that Galton changed his terminology because he now realised the greater generality of the relationship he had found.

Galton did not stop at this confirmation of his earlier result. On examining the joint frequency distribution of offspring and mid-parental heights, he noticed some strange patterns:

I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. They came out distinctly when I 'smoothed' the entries by writing at each intersection of a horizontal column with a vertical one, the sum of the entries of the four adjacent squares, and using these to work upon. I then noticed... that lines drawn through entries of the same value formed a series of concentric and similar ellipses ...
(1885c, 254-5)

Galton guessed that these patterns might be the clue to a deeper understanding of regression. They might, for example, help him understand why, when he reversed the direction of his analysis and examined the distribution of mid-parental heights for a given offspring height, he found a relationship of regression, but with a coefficient of $1/3$ and not $2/3$ (1885a, 509).

Galton decided to try to construct, from what he knew of regression, an equation for the joint frequency surface which had displayed these elliptical patterns. Doubting his own mathematical powers, he sought the assistance of the Cambridge mathematician, J.D. Hamilton Dickson. In formulating the problem for Hamilton Dickson, Galton in fact more or less solved it. Hamilton Dickson was able to write down directly the equation Galton needed (Galton, 1886, 63-6).

Galton's/...

Galton's statement of the problem can be presented in modern terminology and notation as follows. Let y represent mid-parental height, and x offspring height, where both y and x are measured from the means of their respective generations. Suppose that y is normally distributed with standard deviation σ_2 . Let the probability density of y be $g(y)$. Then

$$g(y) dy = \frac{1}{\sqrt{2\pi} \sigma_2} \exp \left\{ -\frac{y^2}{2\sigma_2^2} \right\} dy.$$

Consider now the offspring of those mid-parents with a particular height y . These offspring have mean height $\beta_{12}y$, where β_{12} is Galton's (1877) coefficient of reversion, b. Further, this array of offspring has a standard deviation independent of y ; again this is a result originally formulated in 1877. Call this standard deviation $\sigma_{1.2}$. The conditional probability density of x , given y , is thus

$$f(x | y) dx = \frac{1}{\sqrt{2\pi} \sigma_{1.2}} \exp \left\{ -\frac{(x - \beta_{12}y)^2}{2\sigma_{1.2}^2} \right\} dx.$$

To obtain $h(x, y)$, the joint distribution of x and y , all Hamilton Dickson had to do was to multiply the conditional probability density of x , given y , by the probability density of y .

$$\begin{aligned} h(x, y) dx dy &= f(x | y) dx g(y) dy \\ &= \frac{1}{2\pi\sigma_{1.2}\sigma_2} \exp \left\{ -\frac{(x - \beta_{12}y)^2}{2\sigma_{1.2}^2} \right\} \exp \left\{ -\frac{y^2}{2\sigma_2^2} \right\} dx dy \end{aligned}$$

=/...

$$= \frac{1}{2\pi\sigma_{1.2}\sigma_2} \exp \left\{ -\frac{1}{2} \left[\frac{(x - \beta_{12}y)^2}{\sigma_{1.2}^2} + \frac{y^2}{\sigma_2^2} \right] \right\} dx dy.$$

The contours of equal frequency are given by

$$\frac{(x - \beta_{12}y)^2}{\sigma_{1.2}^2} + \frac{y^2}{\sigma_2^2} = \text{Constant}$$

and are, as Galton had found empirically, ellipses.

The joint probability density can then be factored differently, so that it represents the conditional probability density of y , given x , multiplied by the probability density of x . An expression for the ratio of β_{21} (the regression of mid-parents on offspring) to β_{12} (the regression of offspring on mid-parents) can then be found:

$$\frac{\beta_{21}}{\beta_{12}} = \frac{\sigma_2^2}{\sigma_1^2}$$

Now σ_2 , the standard deviation of the mid-parental generation, is, as shown above, $\sigma_1/\sqrt{2}$. So $\beta_{21}/\beta_{12} = \frac{1}{2}$, and if $\beta_{12} = \frac{2}{3}$, then $\beta_{21} = \frac{1}{3}$, precisely as Galton had found empirically. (25)

Galton had thus, with the assistance of Hamilton Dickson, constructed an expression for what would now be called the bivariate normal distribution. The modern reader/...

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- (25) In the notation used by Hamilton Dickson, $\beta_{12} = \tan \theta$, $\beta_{21} = \tan \phi$, $k\sigma_2 = a$, $k\sigma_1 = c$, $k\sigma_{1.2} = b$, where $k = 0.6745$, the 'conversion factor' from standard deviations to probable errors on the assumption of a normal distribution. In the numerical example (Galton, 1886, 63-4), $\tan \theta = \frac{2}{3}$, $a = 1.22$ inches, $b = 1.50$ inches.

reader may not immediately recognise it as such; that is because the formula is now usually written in terms of r , the coefficient of correlation of x and y . To modern eyes, the step from 'regression' to 'correlation' seems an obvious one. But Galton had no immediate motivation to extend his analysis. His eugenic researches had thrown up specific puzzles, which he had, in his eyes, adequately solved. It was to take a further impetus to make him move from 'regression' to 'correlation'.

The stimulus that led to his work on correlation was a system of personal identification, proposed by the French anthropometrician and criminologist Alphonse Bertillon, which consisted in compiling measurements of selected parts of the body. Galton's interest in the topic of personal identification (another product of which was of course the finger-print system) was in part the result of his general concern with heredity and family likeness:

... one of the inducements to making these inquiries into personal identification has been to discover independent features suitable for hereditary investigation ... it is not improbable, and worth taking pains to inquire whether each person may not carry visibly about his body undeniable evidence of his parentage and near kinships.
(Galton, 1888a, 202)

Bertillon's system was clearly of importance, but one aspect of it worried Galton. Its effectiveness would be reduced to the extent that the component measurements of the system were not independent:

The/...

The bodily measurements are so dependent on one another that we cannot afford to neglect small distinctions. Thus long feet and long middle-fingers usually go together ... No attempt has yet been made to estimate the degree of their interdependence. I am therefore having the above measurements (with slight necessary variation) recorded at my anthropometric laboratory for the purpose of doing so. (Galton, 1888a, 175)

Galton would indeed have been familiar with the well-known biological principle of the interdependence (or correlation) of organs. Thus Darwin had written in The Variation of Animals and Plants under Domestication:

All the parts of the organisation are to a certain extent connected or correlated together; but the connection may be so slight that it hardly exists, as with compound animals or buds on the same tree. (1868, 2, 319)

In his copy Galton underlined the words 'are to a certain extent' and 'so slight'.⁽²⁵⁾ With the mass of data from the Anthropometric Laboratory (as well as Bertillon's own measurements), Galton was in a position to investigate the exact extent of the correlation of various parts of the human body. The results of the investigation (which examined such measurements as stature and cubit) were for him a happy surprise. As he told the Anthropological Institute on 22 January 1889:

... it became evident almost from the first that/...

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(25) Galton's copy is in the library of the Galton Laboratory. Ruth Cowan notes the underlining (1972a, 526).

that I had unconsciously explored the very same ground before. No sooner had I begun to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature, which were submitted to you two years ago. A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect.
(Galton, 1889a, 403-404)

The previous month he had presented the results of his work to the Royal Society:

'Co-relation or correlation of structure' is a phrase much used in biology, and not least in that branch of it which refers to heredity, and the idea is even more frequently present than the phrase; but I am not aware of any present attempt to define it clearly, to trace its mode of action in detail, or to show how to measure its degree.
(Galton, 1888b, 135)

Galton had found that height regressed on cubit, and cubit on height, in the same way as offspring height regressed on mid-parental height. If height and, say, left cubit are both measured from their respective population means, then the mean cubit of individuals with height x would be $\beta_{21} x$, where β_{21} is a constant. Similarly individuals with cubits y would have mean height $\beta_{12} y$. So far so good. But β_{12} is not in general equal to β_{21} , so neither β_{12} nor β_{21} can serve as a measure of the dependence or correlation of height and cubit. The measure must, intuitively, have a property of recipricocity: the correlation/...

(26) Galton did not consider in this paper the possibility of negative values of β .

correlation of height and cubit must be the same as the correlation of cubit and height. As Galton already knew, the lack of recipricocity of coefficients of regression was due to the different probable errors of the two variables involved ($\beta_{21}/\beta_{12} = \sigma_2^2/\sigma_1^2$). Thus the next step was easy, but only once Galton had been motivated to make it.

These relations [of regression] are not numerically reciprocal, but the exactness of the co-relation becomes established when we have transmuted the inches or other measurement of the cubit and of the stature into units dependent on their respective scales of variability. We thus cause a long cubit and an equally long stature, as compared to the general run of cubits and statures, to be designated by an identical scale-value. The particular unit that I shall employ is the value of the probable error of any single measure in its own group.
(Galton, 1888b, 136)

After each variable had been reduced to standard units by division by its own probable error, Galton found that some simple relationships held. Either variable regressed linearly on the other, and the coefficients of regression were equal. This latter result followed of necessity from his procedure, but Galton confirmed it empirically. Galton called the mutual value of the coefficients of regression r . The value of r would always be less than one, he claimed.⁽²⁶⁾ Finally, Galton concluded that ' r measures the closeness of the co-relation' (1888b, 145).

Much/...

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(26) Galton did not consider in this paper the possibility of negative values of r .

Much work, of course, remained to be done on the theory of correlation: for example, the invention by Pearson (1896) of an efficient non-graphical method of calculating the coefficient of correlation. The essential breakthrough, however, had been made. Even though direct eugenic concerns were not present in Galton's work on correlation, the motive to do that work came from an interest in personal identification partly inspired by eugenics, and the intellectual tools used by Galton - the theory of reversion/regression and of the bivariate normal distribution - had themselves been created directly out of Galton's eugenic researches. Galton's eugenics thus accounts at least in part for his invention of correlation.

2.4 Galton and the Error Theorists

The preceding two sections have shown the detailed interconnections of Galton's statistics and his eugenics: the way that at all stages his eugenics informed and guided his statistical theorising. The closeness of this connection is sufficient to suggest that it is reasonable to see Galton's eugenics not merely as providing the motive for his statistical work, but also as conditioning the content of it. Yet a study of his work alone is not sufficient to establish this latter point, for the objection could always be raised that others working at the time might well have developed the same theories, even though/...

though they had no eugenic concerns. It is thus necessary to enquire a little more deeply: to compare Galton's work with that which most closely approached it, and to seek the crucial differentiating cognitive interests.

The other work that came closest to Galton's was done within the error theory tradition. While most error theory dealt with errors in the measurement of one quantity, on occasion two or more simultaneously occurring errors were considered: for example, in the measurement of the position of a point on a plane or in space. Take the case of a point in a plane, whose position is measured with respect to two axes at right angles to each other. Let the errors in measuring one co-ordinate be denoted by x , and in measuring the other co-ordinate by y . Assume that both x and y follow the 'law of error', with probable errors given by $0.6745 \sigma_1$ and $0.6745 \sigma_2$ respectively. Then the probability densities of x and y are given by

$$f(x) dx = \frac{1}{\sqrt{2\pi} \sigma_1} \exp \left\{ -\frac{x^2}{2\sigma_1^2} \right\} dx$$

$$\text{and } f(y) dy = \frac{1}{\sqrt{2\pi} \sigma_2} \exp \left\{ -\frac{y^2}{2\sigma_2^2} \right\} dy.$$

If it can be assumed that the errors in the two directions are independent, then the joint distribution of x and y will be given by

$$h(x,y) dx dy = f(x) dx g(y) dy = \frac{1}{2\pi\sigma_1\sigma_2} \exp \left\{ -\frac{1}{2} \left[\frac{x^2}{\sigma_1^2} + \frac{y^2}{\sigma_2^2} \right] \right\} dx dy$$

This/...

This much was well understood by all error theorists and there is no approach here to the notion of correlation. Several writers in the error theory tradition, however, went beyond this: for our present purposes the most important of these were Bravais and Schols.⁽²⁷⁾

Auguste Bravais (1811-63) worked as a naval officer, astronomer and physicist.⁽²⁸⁾ In his paper (Bravais, 1846) he considered the situation where the position of a point is not determined by direct measurement of its co-ordinates, but where estimates of its co-ordinates are derived from other, more basic, observations. These basic observations (m, n, p, \dots) are mutually independent; the resultant estimates of the co-ordinates of the point (x, y) are, however, not independent as the same basic observation may be employed in the estimation of both x and y .

La coexistence des mêmes variables m, n, p, \dots dans les équations simultanées en x et y , amène une corrélation telle, que les modules h_x, h_y cessent de représenter la possibilité des valeurs simultanées de (x, y) sous le vrai point de vue de la question.
(Bravais, 1846, 263)

The/...

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(27) Robert Adrain appears to have been the first mathematician to consider the simultaneous occurrence of two errors (Walker, 1928, 467-8). Gauss (K. Pearson, 1920; Seal, 1967) and Giovanni Plana (Walker, 1928, 470-6) dealt with simultaneous errors; neither, however, approached the problem of dependent variables as clearly as Bravais and Schols. Bravais's work seems to have become known to British statisticians through the references to it in Czuber (1891); Seal (1967) drew attention to the work of Schols.

(28) For biographical details see Walker (1928) and the Index Biographique des Membres et Correspondants de l'Académie des Sciences.

The 'modules d'erreur' of x and y , h_x and h_y , are, in modern notation, $1/(2\sigma_1^2)$ and $1/(2\sigma_2^2)$: Bravais was noting that the expression

$$\frac{1}{2\pi\sigma_1\sigma_2} \exp \left\{ -\frac{1}{2} \left[\frac{x^2}{\sigma_1^2} + \frac{y^2}{\sigma_2^2} \right] \right\} dx dy$$

does not give the joint distribution of x and y , because of the 'corrélacion' between these two variables.

Bravais showed that - assuming the equations relating x and y to the basic observations to be linear, and the basic observations to be mutually independent and to follow the 'law of error' - the joint distribution of x and y was

$$\frac{K}{\pi} \exp \left\{ -[ax^2 + 2exy + by^2] \right\} dx dy,$$

where K , a , b and e were constants to be evaluated. 'Le problème se trouve ainsi ramené à déterminer, a posteriori, la valeur des quatre coefficients K , a , b , e ' (Bravais, 1846, 268-9).

Bravais had thus reached an equation formally very similar to that of Galton and Hamilton Dickson. The modern statistician, acquainted with Galton's work on the bivariate normal surface, would expect Bravais to continue by evaluating his constants in terms of the coefficient of correlation of x and y or of the coefficients of regression. Bravais in fact did nothing of the sort. Passing through an intermediate stage of the analysis where he evaluated the/...

the separate 'modules d'erreur' of x and y in terms of K , a , b and e (Bravais, 1846, 269-70), Bravais went on to construct expressions for K , a , b and e in terms of the parameters of the linear transformation by which x and y were derived from the direct observations and the 'modules d'erreurs' of these observations (Bravais, 1846, 270-2). He made no attempt to investigate or measure the dependence of x and y in his further analysis, the most interesting aspects of which were his discussion of the elliptical contours of equal frequency, and his extension of the result to three variables, x , y and z .

Charles Schols (1849-97) worked as a civil engineer and taught at the Breda military academy before being appointed Professor of Geometry at the Polytechnic of Delft.⁽²⁹⁾ Unlike Bravais, who worked with a model of independent basic observations, Schols was prepared to consider dependent basic variables. He criticised previous writers who had assumed the independence of errors in different directions in treating problems of artillery fire:

Dans les principaux ouvrages qui traitent de la probabilité du tir, les formules ... sont établies à l'aide du théorème de la probabilité composée d'événements indépendants les uns des autres en multipliant les probabilités des déviations dans les deux directions. Dans cette déduction on n'a pas fait attention à ce que ces deux déviations ne sont pas indépendantes l'une de l'autre.
(1886, 174)

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(29) Biographical details for Schols are taken from Poggendorf's Biographisch-Literarisches Handwörterbuch.

Schols worked with a model he claimed (1886, 176) to be more general than that of Bravais: he assumed only that the errors of the co-ordinates of a point in space were the resultant of a number of small errors.

Schols followed a process of analogical reasoning, treating the distribution of error by analogy with the inertia of a rigid body, and showing that probable errors corresponded to moments of inertia. He concluded that the distribution of error would have principal axes similar to those of the ellipsoid of inertia, and showed that with respect to these principal axes, the law of error could be written as (in modern terminology):

$$f(x,y,z) dx dy dz = \frac{1}{(2\pi)^{3/2} \sigma_1 \sigma_2 \sigma_3} \exp \left\{ -\frac{1}{2} \left[\frac{x^2}{\sigma_1^2} + \frac{y^2}{\sigma_2^2} + \frac{z^2}{\sigma_3^2} \right] \right\} dx dy dz.$$

In other words (Schols, 1886, 149):

La résultante d'un grand nombre d'erreurs suit la même loi que la résultante de ses trois projections sur les axes principaux, considérées comme indépendantes les unes des autres.

Schols's conclusion was that although the assumption of the independence of errors in different directions was not necessarily correct, one could act as if it were correct, provided one worked with the correct system of axes:

Lorsqu'on veut appliquer le théorème de la probabilité d'événements indépendants, on ne peut le faire qu'après avoir démontré que les écarts peuvent être considérés comme indépendants, bien qu'ils ne le soient pas en réalité, démonstration que nous avons donnée ...
(Schols, 1886, 175; Schols's emphasis)

From/...

From the point of view of a realist theory of statistics (see section 1.3), the behaviour of Bravais and Schols seems very strange. Why did they not 'discover' correlation or regression? Bravais explicitly and Schols implicitly⁽³⁰⁾ had reached an expression formally identical to that derived by Galton; being better mathematicians than he, they were even able to deal with the three variable, and not merely the two variable, case. While Bravais might be 'excused' on the grounds that, as Pearson (1920b, 192) points out, he simply assumed that basic observations must be uncorrelated, this does not, as Seal (1967, 219) notes, apply to Schols.

When, however, a non-realist perspective is adopted and the cognitive interests structuring error theory are considered, this puzzle disappears. Both Bravais and Schols were, despite the somewhat abstract nature of their papers, dealing with practical problems of error theory. In Bravais's case, the particular problem appears to have been theodolite work (Pearson, 1920b, 190); in that of Schols, artillery fire. In both cases, cognitive interests in the control and measurement of error are apparent: Bravais and Schols were attempting to provide two-dimensional and three-dimensional analogues of the successful one-dimensional technology of the law of error and/...

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(30) Had he 'bothered' to write his equation with respect to any set of axes other than the principal axes, that is,

and the probable error. As stated above, most error theorists felt quite justified in treating the problem by assuming the independence of errors in different directions. As it is part of good experimental or observational technique to ensure that different measurements are independent of each other, this was a perfectly reasonable assumption to make. Bravais's procedure on having reached his two-dimensional law of error makes perfect sense in this perspective. The 'corrélacion' of x and y was the result simply of the fact that x and y were constructs from the basic observations. Bravais was not concerned to examine their dependence from the point of view, say, of the influence of one on the other, for this would have made little sense. Instead he worked back to what he knew about, the basic observations and their probable errors, in order to be able to express the probable errors of x and y and their law of error in terms of empirically known quantities.

For Schols, too, the problem of the dependence of variables was a 'residual' one to be analysed away. Working in a marginal area to which error theory applied only by extension, he felt unable to make the confident assumption of the independence of the basic variables. An astronomer or surveyor whose basic measurements were correlated could be told to improve his technique so as to produce independent variables, but an artillery captain might well not be able or willing to do so. Schols's solution was to show that even if the problem might be substantively/...

substantively intractable, a neat mathematical device, the use of principal axes, solved it theoretically by 'dissolving' the dependence of the variables. Having shown this, he was content. Writing out his equation for axes other than the principal axes was irrelevant, as was any further investigation of the dependence of x , y and z .

In the language of Kuhn (1970), it could be said that Bravais and Schols were extending the basic paradigm of error theory to solve the problem of dealing with dependent variables, a problem which was marginal rather than central to the theory. They provided solutions that were practically adequate but involved no new concepts. Galton, on the other hand, was working in a completely different framework. As a eugenicist, he was naturally concerned with the effect of the characteristics of one generation on that of the next. The statistical dependence of two variables (of, say, offspring height and mid-parental height) was thus central to his research. Statistical dependence was no marginal problem to Galton: it was the very basis of the possibility of a eugenic programme, for eugenics would be impossible if parental and offspring characteristics were independent. Galton's eugenics thus gave rise to a cognitive interest, absent in error theory, in the understanding and measurement of statistical dependence as a phenomenon in its own right. His conceptual innovations, the statistical notions of regression and correlation, reflect this interest. What differentiated Galton's/...

Galton's theoretical work from that of the error theorists was thus a cognitive interest arising from his eugenic concerns. In this sense we can conclude that eugenics entered into his development of statistical theory by providing a new problem, as well as by providing in the concept of reversion/regression - taken over into statistics from his eugenically-informed biology - the resources necessary for its solution.

between eugenics and statistics was intimate. In order to achieve a rounded view of the development of statistical theory in this period, it is therefore necessary to set it in context by discussing the evolution and social nature of the British eugenics movement. The sociological analysis of eugenics developed in this chapter will be used below, especially in chapters four, six and seven, in an attempt to analyse particular intellectual developments in terms of the sociology of knowledge.

A few introductory words are in order. 'Eugenics' referred, primarily, to schemes for 'racial improvement' by deliberate legislative or other attempts to alter the social distribution of fertility. The two strategies considered were that of increasing the fertility of the 'fit', which was known as positive eugenics, and decreasing the fertility of the 'unfit', or negative eugenics. Closely bound up with eugenics as a social programme was an account of human biology that claimed that certain crucial human characteristics (such as mental ability) were largely inherited/...

Chapter Three

Eugenics in Britain

Eugenics forms the backdrop to many of the developments in statistical theory discussed in this thesis, and the case of Francis Galton indicates that the connection between eugenics and statistics was intimate. In order to achieve a rounded view of the development of statistical theory in this period, it is therefore necessary to set it in context by discussing the evolution and social nature of the British eugenics movement. The sociological analysis of eugenics developed in this chapter will be used below, especially in chapters four, six and seven, in an attempt to analyse particular intellectual developments in terms of the sociology of knowledge.

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inherited and relatively little affected by environment. I shall normally refer to this account by its usual modern name of 'hereditarianism'. The development of the hereditarian account of human beings and the study of the social distribution of fertility, its consequences and the factors affecting it, formed the 'scientific' aspect of eugenics.⁽¹⁾

Unlike its counterpart in the United States,⁽²⁾ the eugenics movement in Britain has only very recently begun to receive the attention of historians. With the exception of some work by members of the Eugenics Society (Blacker, 1952; Schenk and Parkes, 1968), the only important secondary source was for several years Farrall (1970). Farrall's comprehensive and detailed study firmly established the essential points of the history of eugenics in Britain, and it is extensively drawn on here. Since the completion in 1975 of the original draft of this chapter (which appeared/...

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- (1) To introduce a systematic distinction between a 'scientific' side of eugenics and eugenics as a social programme would, however, be anachronistic and misleading. Both are linked in Galton's original definition:

We greatly want a brief word to express the science of improving stock, which is by no means confined to questions of judicious mating, but which, especially in the case of man, takes cognisance of all influences that tend in however remote a degree to give to the more suitable races or strains of blood a better chance of prevailing speedily over the less suitable than they otherwise would have had. The word eugenics would sufficiently express the idea ...
(Galton, 1883, 25)

- (2) For eugenics in the United States see Haller (1963), Ludmerer (1972), Pickens (1968) and Allen (1975b; 1976).

appeared as MacKenzie, 1976) two further works have become available. Waterman (1975) discusses the Eugenics Society in the decade before the Second World War, and thus his concerns are rather different from those of this chapter. Nonetheless, his views on the causes of the last phase of the decline of the eugenics movement are not contradictory to those advanced here. Searle (1976) discusses the period 1900 to 1914. His description of eugenics before the First World War is fully congruent with that advanced here, although it is arguable that he does not develop his analysis to its full potential extent (MacKenzie, 1978b).

3.1 Eugenics and British Culture

As pointed out in section 1.3, social interests always operate within a framework of pre-existing culture. British eugenisists did not develop their ideas in an intellectual vacuum. They drew on already present beliefs about heredity and society, adopting some of these unaltered, transforming others to suit their interests, and adding new elements. While the result was undoubtedly a new set of ideas, it is worth briefly examining some of the cultural resources on which they drew.

In his biography, Galton (1908, 288) described early nineteenth century beliefs about heredity as 'lax and contradictory'. To the extent that this was so, it can/...

can be attributed to the large variety of social purposes that such beliefs served. The animal breeder used heredity as a guide in developing stock; the physician used it as an explanation of disease; the moralist used it to sanction deviance; the middle class male used it as an argument for female passivity (Rosenberg, 1974). Before eugenics, there was no single dominant social use to which heredity was put, there was no generalised controversy about heredity, and thus there was little pressure to consistency in the deployment of ideas. 'Clarification' came only as a result of the eugenicists' systematic and controversial use of the ideas of heredity; pre-eugenic notions formed a rich, varied and plastic body of knowledge capable of easy deployment in various directions.

Hereditarian beliefs were employed in arguments about social reform before eugenics, but the use made of them was frequently opposite to that typical of the eugenics movement. Heredity was invoked as a sanction reinforcing the case for particular environmental reforms. Bad conditions, drunkenness and drug abuse were held to have a detrimental effect on the children of the present generation, through the inheritance of acquired characteristics. Environmental reforms - such as sanitary improvements or a curb on the drink trade - would, it was claimed, improve not simply this generation but the next.⁽³⁾ As Rosenberg (1974/...

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(3) It should, however, be noted that many of those campaigning for compulsory custodial treatment of alcoholics did not accept this optimistic view, and saw retreats as a means of isolating alcoholics. See R.M. MacLeod (1967a).

(1974, 221-2) points out, Richard Dugdale's famous study of the Jukes family was not a call for eugenics, as it was later to be interpreted, but for environmental reform. Sufficiently vigorous action in education and the improvement of conditions, extended over two or three generations, could, it was hoped, stamp out the social evils manifested by the Jukes family.

It is not possible to attribute the change in the social uses of beliefs about heredity that took place in the later nineteenth century simply to internal changes within science. Certainly, most British biologists after 1890 did follow August Weismann in his rejection of the view that acquired characteristics could be inherited. And eugenists did use this as a basis for arguing that only eugenic reform could have a permanent effect on the race. However, it is clear that Weismannism did not cause eugenics. Galton had independently rejected the inheritance of acquired characters before Weismann's work appeared, possibly because of his eugenic views (Cowan, 1968 and 1977, 156-7). The reception of Weismann's views in Britain was, in fact, strongly conditioned by their perceived political significance.⁽⁴⁾ There had been no major change in the available scientific evidence on the inheritance of acquired characteristics. Nor did acceptance/...

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(4) See, for example, Ball (1890): Burnham (1972) argues that this political response to Weismann was a peculiarly Anglo-American phenomenon.

acceptance of Weismannism compel or even indicate advocacy of eugenics.⁽⁵⁾

Another component in the intellectual background of eugenic thought was political economy and the image of society it developed (Abrams, 1968). For all its rejection of Enlightenment optimism, of the environmentalism of the utilitarians, and of the revolutionary-bourgeois notion that 'all men are born equal', eugenics retained certain key elements of classical bourgeois thought. The eugenic view of society was individualistic and atomistic. The fitness of a society was the sum of the fitnesses of the various individuals comprising it. Although the eugenicists stressed race, their view of race was not a holistic one. A race was not an unalterable essence, but a historical population, the sum of its parts.

There was a particularly close affinity between eugenics and the biological variant of bourgeois political economy, social Darwinism. The eugenic identification of social failure with biological unfitness, the notion of progress coming through the elimination of the 'unfit', and the biological view of society, are all drawn from social Darwinism. Indeed, Halliday (1971) has attempted to treat the two movements as more or less equivalent. In this/...

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(5) Alfred Russel Wallace, for example, accepted that acquired characteristics were not inherited, but rejected eugenics as a political programme. See Wallace (1890).

this, however, he is wrong. Earlier social Darwinism (especially Spencer's) held that the elimination of the unfit could be achieved by political inaction. If the state would stop interfering in the working of natural laws, all would be well (Spencer, 1873, 343-6). Eugenics, in contrast, did not trust to laissez-faire. 'What Nature does blindly, slowly, and ruthlessly', wrote Galton (1909, 42), 'man may do providently, quickly, and kindly'.

Thus, eugenicists drew on resources present in the culture of Victorian Britain. They combined these in their own characteristic manner and developed from them patterns of thought of a novel kind: both general, such as the nature/nurture distinction, and more specialised, such as the statistical view of heredity and evolution. We must now consider who developed and propagated this new and characteristic body of thought.

3.2 The Social Composition of the Eugenics Movement

British eugenics can, for our purposes, be said to have begun in the 1860's with the publication of the first article on the subject by Galton (1865). During the 1880's eugenics became a definite topic of public discussion in books and articles. Between 1900 and 1914 it achieved institutional expression, notably with the establishment of a Eugenics Laboratory in the University of London and in 1907 with the foundation of the Eugenics Education/...

Education Society (E.E.S.). By 1913-14 the E.E.S. had over 1,000 members (Farrall, 1970, 211-2).

The most straightforward answer to the question, 'Who were the eugenists?', is provided by examining the membership of the E.E.S. in the key years 1908-14. With some exceptions (notably Karl Pearson), nearly all known British eugenists were members of the Society. Its membership has been examined by Farrall, who concludes (1970, 225-8):

The leadership of the Eugenics Education Society was dominated by well-educated members of the middle-class professions of medicine, university teaching and science ... Membership was not only drawn almost exclusively from the middle classes but also heavily from the intellectual, creative and welfare professions. Of those whose profession has been discovered only three military officers and one businessman would be excluded definitely from this category.

To the extent that the hypothesis of membership drawn virtually exclusively from the professional middle class is true, it should be possible to identify every member of the E.E.S. by use of the various biographical dictionaries of the professions (such as the Medical Directory), in addition to sources such as the Dictionary of National Biography and Who's Who. As a check that this could in fact be done, and that the rather high proportion of individuals not positively identified by Farrall did not contradict his conclusion, I examined one group of members: the forty-one elected members of the Council for 1914 (vice-presidents/...

presidents and honorary members were omitted). Forty of these were identified (see appendix B for details), and their occupations were as follows:

| | |
|--|----------------|
| University teachers and researchers | 11 |
| Doctors ⁽⁶⁾ | 9 |
| Lawyers | 4 |
| Politicians ⁽⁷⁾ | 2 |
| Non-university scientists ⁽⁸⁾ | 2 |
| Writers | 2 |
| Headmasters | 1 |
| Clergymen | 1 |
| Other ⁽⁹⁾ | 8 |
| | <hr/> 40 <hr/> |

This confirms Farrall's analysis. It seems safe to conclude that while eugenics may possibly have enjoyed support amongst other social groups, the bulk of its activists were of the professional middle class. Business and/...

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- (6) Several of the doctors held university or medical school teaching posts.
- (7) One, Cockburn, a doctor by training.
- (8) One, Havelock Ellis, also an author; the other, Mond, also a businessman.
- (9) One official of the National Association for the Welfare of the Feeble-Minded, one farmer (and amateur agricultural scientist), one retired army engineer, and five wives or widows (of a naval lieutenant, an admiral, a civil servant and businessman, a merchant and geographer, and a surgeon) for whom no occupational data could be found.

and the hereditary aristocracy (as distinct from ennobled commoners) were not prominent in support of eugenics, in or out of the E.E.S. Nor were the working class. It would also seem clear that the eugenists were not recruited equally from all sections of the professional middle class. The universities, science and medicine were heavily represented; law and the church more sparsely. Finally, it is of interest to note that a large proportion of the members of the E.E.S. were women; this was not, however, fully reflected in the composition of its Council.⁽¹⁰⁾

Such evidence on social composition is, however, inevitably ambiguous. One possible conclusion from it is that drawn by Farrall, who identifies eugenics as a form of the 'middle class radicalism' described by Parkin (1968). Members of the 'welfare and creative' professions tend to join reforming bodies for reasons which reflect, not their social interests, but the psychological satisfaction to be found in moral reform. Thus

... the members of the eugenics movement found emotional satisfaction in expressing their personal beliefs in action rather than seeking specific material improvement in their status within society.
(Farrall, 1970, 293)

On this view, there is no reason to expect any intimate connection between the composition of the eugenics movement and/...

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(10) The complex relations of eugenics and feminism will be discussed by Helen Rugen in her forthcoming University of Edinburgh Ph.D. thesis.

and the views it put forward, other than the fact that these were of a general moral-reforming nature. Another conclusion, however, is also possible: that eugenics was an ideology of the professional middle class, and that it did reflect the search of members of this group for 'material improvement in their status within society'. On this view, eugenics would be not simply a movement of professionals, but also a movement for professionals. My reading of the propaganda and schemes of the eugenicists supports this latter view.⁽¹¹⁾ Before discussing this, it is however necessary to turn briefly to some relevant features of the social position of the professional middle class.

3.3 The Professional and the Social Structure

On a Marxist view of industrial capitalist societies, the basic capital/labour polarity gives rise to two fundamental classes, the bourgeoisie and the proletariat. This dichotomy must, however, be refined in order to examine the actual social structure of Victorian and Edwardian Britain with a view to discussing the eugenics movement. Firstly, it is necessary to note the clear distinction drawn by contemporaries between what were sometimes referred to as/...

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(11) See also Searle (1976, 45-66), who comments that 'the professional middle classes and the intelligentsia' were 'the heroes of the play' in eugenic propaganda (59).

as the 'respectable working class' and the 'residuum' or lumpen-proletariat. This distinction will be returned to in section 3.5. Secondly, attention must be given to the significant number of occupational positions that can be classed neither as 'bourgeois' nor as 'proletarian'. Professional jobs fall into this category. Some professionals such as doctors and lawyers were self-employed, and can perhaps be seen as, in the traditional sense, petty bourgeois. An increasing number of professionals were, however, employees of the state or private concerns. Like the proletariat, they were forced to sell their labour power, even though the terms on which they did so were much more favourable.

The group of self-employed and salaried professionals form what is usually referred to as the 'professional middle class'.⁽¹²⁾ Certain general features of the/...

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(12) There would be little point in embarking here on a discussion as to whether or not professionals and other white-collar workers form a class in the Marxist, or any other, sense. That issue - together with the consequent problems of what class it is, if they form a class, or to which class they are attached, if they do not - is the subject of much debate. See for example Poulantzas (1975), Carchedi (1975a; 1975b; 1976), B. and J. Ehrenreich (1976) and Johnson (1977). For the limited purposes of the present discussion it is sufficient to draw on this literature simply for the insights it provides into the social situation of the professional. The following analysis does not assume the validity of any particular (Marxist or non-Marxist) class theory, although it might eventually profit from reformulation in terms of an adequate theory of the professional middle class, should such a theory be developed.

the social situation of professionals can be identified, at least tentatively. Firstly, they occupy a position intermediate between the bourgeoisie and the proletariat. They are differentiated from the bourgeoisie (and the aristocracy) in that they do not own or control substantial quantities of capital (or land). They are differentiated from the proletariat in that their work is held to be mental labour, superior to manual labour. Secondly, recruitment to this group is generally not automatic, but is achieved through the education system. As B. and J. Ehrenreich (1976, 29) point out, the son of a wealthy businessman is virtually ensured the possibility of a similar position; the same holds for the son of a manual labourer. 'Class reproduction' for professionals is, however, more precarious. Being the child of a professional is an advantage, but does not guarantee similar status. B. and J. Ehrenreich suggest that 'class reproduction' comes to dominate the 'interior life' of the class:

... all of the ordinary experiences of life - growing up, giving birth, childraising - are freighted with an external significance unknown in other classes.
(1976, 29)

While this may be exaggerated (the degree of this anxiety must undoubtedly be historically variable), it is worth noting that 'class reproduction' takes on a specifically problematic form in the professional middle class.

Thirdly, the absolute and relative number of professional jobs, and their social importance, tend to increase with the/...

the development of capitalist economies. This process is, however, not uniform. Thus Hobsbawm (1968, 267) suggests that, in some respects, it was relatively slower in Britain than in Germany or France. Further, it is at least arguable that these jobs and the fortunes of their incumbents are not tied to the continuance of a specifically capitalist economic order, and that the responsibilities and rewards associated with them might in at least certain cases be no less, or even greater, in a socialist state.

What strategies does the professional middle class typically employ in pursuit of its interests? Historically, the most significant has of course been 'professionalisation' itself. The rationale of professionalisation is to legitimate the activity of an occupational group by reference to its accredited possession of a body of knowledge, to impose controls on access to this knowledge and to membership of the occupational group, and to free the group as much as possible from pressure from outsiders or 'laymen'. The strategy of professionalisation thus reflects the crucial role of accredited knowledge in differentiating the professional middle class from the bourgeoisie and the proletariat. Professional autonomy and control over access to membership of the profession are also important in alleviating the difficulties of 'class reproduction'. The high rate of self-recruitment to be found in the medical profession, for example, is evidence of the degree to which this strategy can bear fruit. When we turn to more general/...

When we turn to more general political strategies, we find, however, a certain indeterminateness. The professional middle class is a relatively privileged group within capitalist society, and yet many professional jobs are not bound intrinsically to a capitalist order. A professional's conservatism and a professional's socialism are both possible. What does seem likely, however, is that both the conservatism and the socialism will be expressed in terms of an ideology of the 'expert' and of 'meritocracy'. Harold Perkin writes of the British professional middle class:

Their ideal society was a functional one based on expertise and selection by merit. For them trained and qualified expertise rather than property, capital or labour, should be the chief determinant and justification of status and power in society.
(1972, 258)

Charting the growth of the professional middle class in Britain is difficult. Problems in the occupational classifications in the census, and the general difficulty of deciding when a particular occupation became in the full sense a 'profession', make even rough estimates of numbers difficult.⁽¹³⁾ The formal growth of professionalisation is/...

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(13) Mitchell and Deane (1962, 60) give a table of figures for the period 1841-1921 suggesting that the percentage of occupied males who were in 'professional occupations and their subordinate services' rose from about 2% in 1841 to 3% in 1921, while the corresponding figures for females were 3% in 1841 and 8% in 1921. These bald figures are however misleading. For example, 'professional entertainers and sportsmen' are included, but veterinary surgeons excluded. Even more seriously, the rapid change in the position of particular occupations in this period means that the social content of particular occupational labels completely altered in many cases.

is easier to chart.⁽¹⁴⁾ Perkin comments (1972, 254-5):

With urbanisation and the rise of living standards, doctors, lawyers, writers, and even the clergy (including dissenting ministers) found an enlarged demand for their services, which reduced their dependence on the few rich and increased that on the many comfortable clients of their own social standing. The transition enabled them to acquire a greater measure of self-respect, and to demand corresponding respect from society ... At the same time new professions proliferated, and organised themselves to demand the same kind of status and independence as the old.

The emergence of some of the newer scientifically-based professions was undoubtedly slower in Britain than, say, in Germany, and overall growth in the number of professionals was perhaps less important than consolidation and the drawing of boundaries. Nevertheless, it seems reasonable to talk (at least by the latter part of the nineteenth century) of the emergence of an established professional middle class. Of course, there were important lines of division: between self-employed professionals and the newer group of professional employees; between the older professions such as law and the church, and the new more marginal ones; between male professionals and women seeking, or having succeeded in achieving, entry to the professions. Despite this, it would appear that the professional middle class did have some common sense of identity and social position (Perkin, 1972, 254-61). In the next two sections, we shall see how/...

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(14) The classic study of this, the senior author of which was a prominent member of the Eugenics Society, is Carr-Saunders and Wilson (1933).

how in the case of some professionals this consciousness related to eugenics. First, let us turn to the founder of the eugenics movement, Francis Galton.

3.4 Francis Galton and the Origins of Eugenics

By birth, marriage and inclination Galton belonged to an early élite of the emerging Victorian professional middle class. N.G. Annan (1955) has called the group to which Galton belonged 'the intellectual aristocracy'. The origins of this group lay in the bourgeoisie. The families from which it came were distinguished from the bulk of the bourgeoisie by religion (they were Quakers, Unitarians or members of the Clapham Sect) and by their philanthropic and anti-slavery concerns. The children of marriages within this group tended to abandon direct business involvement for the world of scholarship, education and the professions. They rapidly rose to dominant positions in the universities, public schools, science and literature. Some entered the state bureaucracy, to become 'mandarins' of the increasingly professional civil service. Although ties of kinship and common interest bound this group to other sections of the élite of Victorian Britain, Annan (1955, 248) emphasises that it maintained a separate identity. At least until the end of the nineteenth century, it remained tightly-knit, held together by continuing inter-marriage and by a common commitment to educational and administrative reform, to the abolition of religious tests and/...

and to the introduction of selection by competitive examination in the civil service. This programme expressed the 'intellectual aristocracy's' commitment to modernisation, and laid the basis, in its success, for a growth in the size and influence of the professional middle class as a result of the expansion of education and rational bureaucracy.

Francis Galton could well be taken as an archetype of this group. He was born into one of the families of the 'intellectual aristocracy' (the Wedgwood/Darwin/Galton family) and married into another (the Butlers). He inherited from his Quaker ancestors sufficient money never to have to practise a profession for gain (he was trained in medicine and mathematics), and the two families to which he belonged brought him connections in science, medicine, education and the church. Direct observation of kinship links within this professional élite may have been the source of his initial hereditarian convictions. He wrote in his autobiography (Galton, 1908, 288):

I had been immensely impressed by many obvious cases of heredity among the Cambridge men who were at the University about my own time.

He did not, however, give any general interpretation to this to begin with. The spur to such an interpretation was the publication by his cousin, Charles Darwin, of The Origin of Species (Darwin, 1859). Almost fifty years later, Galton wrote (1908, 287):

The publication in 1859 of the Origin of Species by Charles Darwin made a marked epoch in my own mental/...

mental development, as it did in that of human thought generally. Its effect was to demolish a multitude of dogmatic barriers by a single stroke, and to arouse a spirit of rebellion against all ancient authorities whose positive and unauthenticated statements were contradicted by modern science.

As important as any detailed impact that Darwin's work had on Galton was the general effect on him of the controversy following its publication. Galton was present at the British Association meeting at Oxford in 1860 when Huxley and Wilberforce debated Darwin's theories (Forrest, 1974, 84). Galton clearly felt the need to choose sides between scientific naturalism and its theological opponents. Given his background, there could be little doubt which side he would choose. He became a leading member of the group of scientific intellectuals which included Huxley, Spencer and Tyndall. He vigorously opposed the dogmas of revealed religion, and sought to replace the Christian faith by a system of belief based on natural science. The near monopoly of the church in comfortable professional positions must, Galton felt, be ended, and an adequately-supported profession of science established. The scientists' role should not be a mere technical one: they should form 'a sort of scientific priesthood throughout the kingdom, whose high duties would have reference to the health and well-being of the nation in its broadest sense' (Galton, 1874, 260)⁽¹⁵⁾.

In/...

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(15) For Galton's anti-clericalism and its context, see F.M. Turner (1974b). Turner (1974a) is a sensitive study of the relations of science and religion in this period.

In the 1860's, Galton began to interpret his experience of kinship links in the professional élite in a naturalistic and evolutionary framework, and to derive from this a faith and a social practice for the scientific priesthood. The method of his initial studies in heredity was a simple generalisation of his early observations of his contemporaries. He sought to trace kinship links amongst those acknowledged to be of exceptional mental ability (amongst his examples were his own and his wife's families). By this means he showed that achievement ran in families: the closeness of kinship links amongst the eminent was far greater than would be expected if eminence was distributed at random in the population. This Galton interpreted as proof of the inheritance of mental ability, and he went on to argue for a eugenic programme which would ensure the careful and early marriage and high fertility of the most able (Galton, 1865; Galton, 1869). Galton saw in eugenics the basis for a new scientific and evolutionary religion, in which an individual would be seen only as a manifestation of immortal germ plasm (1869, especially 376).

The practice of eugenics also necessitated social changes. The dominance of society by plutocracy and hereditary nobility must, Galton felt, be ended. Extremes of inherited wealth and titles of nobility had a bad effect on the race, causing the degeneration and sterility of originally healthy stock.

The/...

The best form of civilisation in respect to the improvement of the race, would be one in which society was not costly; where incomes were chiefly derived from professional sources, and not much through inheritance; where every lad had a chance of showing his abilities, and, if highly gifted, was enabled to achieve a first-class education and entrance into professional life, by the liberal help of the exhibitions and scholarships which he had gained in his early youth; where marriage was held in high honour as in ancient Jewish times; where the pride of race was encouraged (of course I do not refer to the nonsensical sentiment of the present day, that goes under that name); where the weak could find a welcome and a refuge in celibate monasteries or sisterhoods, and lastly, where the better sort of emigrants and refugees from other lands were invited and welcomed, and their descendants naturalised. (Galton, 1869, 362)

At the end of his life, Galton wrote a novel, Kantsaywhere, in which he described his eugenic utopia. (16) This reads, in many respects, as a direct description of the practice and ideals of the 'intellectual aristocracy'. The island of Kantsaywhere is dominated by a benevolent oligarchy, the Eugenic College, who administer it along the lines suggested by Galton's early articles. The College examines eugenic fitness, encourages the early marriage of the 'fit', and deports or segregates the 'unfit'. The population have fully accepted the rule of the College, and 'everyone is classed by everybody else according to their estimate or knowledge of his person and faculties'. The College is trusted and looked up to:

The Trustees of the College are the sole proprietors/...

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(16) This was never published. The surviving fragments are reproduced in Karl Pearson (1914-30, 3A, 411-25).

proprietors of almost all the territory of Kantsaywhere, and they exercise a corresponding influence over the whole population. Their moral ascendancy is paramount. The families of the College and those of the Town are connected by numerous inter-marriages and common interests, so that the relation between them is more like that between the Fellows of a College and the undergraduates, than between the Gown and Town of an English University. In short, Kantsaywhere may be looked upon as an active little community, containing a highly-respected and wealthy guild.
(quoted by K. Pearson, 1914-30, 3A, 414)

Competitive examinations determine status, the intellectually gifted intermarry, and the dominance of society by the extremely wealthy and titled has been replaced by the dominance of the intellectual élite. In short, the relaxed social control of the university, passing and 'plucking', has been extended over the whole of society.

Galton's eugenics had thus a double aspect. He came from an intellectual elite closely bound by kinship ties. In this social group achievement was inherited (though we might now want to interpret this socially rather than biologically). Successful fathers had successful sons; these sons generally married within the social group and themselves had successful offspring.⁽¹⁷⁾ So Galton was interpreting generally and naturalistically a salient facet of his social experience. At the same time, an intrinsic part of his eugenic programme was the advancement of the interests of the professional middle class. The middle/...

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(17) Mothers and daughters, it is worth noting, scarcely figured in Galton's eugenic thought except as the transmitters of hereditary ability.

middle class 'expert', rather than the priest, aristocrat or plutocrat, should exercise power in an efficient, modernised, eugenic society. Science, rather than Christian religion, should be the dominant cultural form. Thus Galton's eugenics both expressed the social experience and reflected the social interests of the rising professional élite to which he belonged. (18)

3.5 Eugenics as an Ideology of the Professional Middle Class

Although, as we have seen, the eugenics movement was made up almost exclusively of members of the professional middle class, few were of such high status as Galton. (19) Nor/...

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(18) Buss (1976) interprets the origins of Galton's eugenics differently. He argues that it arose from the contradictions between the liberal individualist emphasis on the existence of equality of opportunity and the facts of the hierarchical division of labour: a hereditarian interpretation of mental ability being necessary to explain why, given equal opportunities, such grossly unequal outcomes could result.

It seems to me that while this view is useful in understanding eugenics in general, there is little evidence that Galton felt the particular contradiction Buss outlines. Further, Buss's account of 'democratic-liberal-capitalistic-individualism' seems idealised, as when he claims (1976, 56):

We see in Kantsaywhere an ideological doctrine of eugenics that was a distortion of reality vis-a-vis British nineteenth century liberal individualism... In Kantsaywhere the political system would seem to be totalitarian ...

(19) Indeed given the closeness of the functional and kinship relations of the 'intellectual aristocracy' to the British bourgeoisie and aristocracy, we might almost want to describe it as a professional, modernising fraction of the British ruling class. This fraction might then be seen as separate from, but exercising hegemony over, the bulk of the professional middle class. Certainly the 'intellectual aristocracy' provided the culture heroes of the British professional middle class from Darwin to Keynes.

Nor did later eugenic methodology reflect the social origins of eugenics in as direct a way as did Galton's kinship studies. Nevertheless, it is possible to see several connections between eugenics and the professional middle class. These connections seem to me to indicate that British eugenics can best be seen as an ideology of the professional middle class. (20)

At times the Eugenics Education Society acted as a straightforward advocate of the financial interest of the middle class:

... the incidence of the income tax is claiming attention, and a letter has been sent by the President to all Members of Parliament pointing out that any system of taxation which takes no account of the necessary expenditure involved in bringing up a family may, in a sense, be said to penalise marriage and parenthood, and that taxation which retards marriage and discourages parenthood on the part of worthy citizens has a harmful influence in tending to lower the proportion of men and women of good stock or blood in the composition of the generations of the future. There is no question that the income tax at present falls most heavily on parents belonging to the middle and professional classes, to whom this description can be appropriately applied.

It is suggested that the way to remedy this evil is to extend the principle of allowing rebates for each child...
(E.E.S., 1914, 7)

When/...

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(20) This analysis is intended to apply only to Britain. Garland Allen (1975b, 39) argues that eugenics in the United States was 'founded, financed or in other ways supported' by at least a section of the ruling class. American eugenics differed in content from British eugenics, in that race, rather than class, appears to have been the dominant theme (Rosenberg, 1974, 227). Allen (1975b, 41) suggests that it was the racial aspect of American eugenics that made it attractive to the American ruling class.

When the First World War broke out, the Council of the E.E.S. discussed what practical eugenic action could be taken in the war situation. As a result of this discussion, the E.E.S., in conjunction with the heads of the leading professional bodies and institutions, helped form a 'Professional Classes War Relief Council' and set up a maternity home for the wives of professional men serving in the armed forces. (E.E.S., 1915, 4-5).

The significance of eugenics was, however, much wider than this. Eugenics was used both to legitimate the social position of the professional middle class and to argue for its improvement. As noted in section 3.3, the professional middle class owes its social position neither to wealth nor to ascribed status, but to the specialised mental abilities and knowledge of its members. The hereditarian theory of mental ability, as developed by the eugenicists, implied that only a limited section of the population had the potential to achieve the skills and knowledge required for professional middle class roles. The professional middle class had, according to the eugenicists, achieved their position, not by accident of circumstances, but as the result of generations of selection for mental ability. The next generation of professionals would of necessity have to be recruited largely from the existing professional middle class. Thus, a rigidly stratified educational system was justified, with only the narrowest of ladders to allow the unusually gifted child, the/...

the 'sport', to rise from the lower classes. Eugenics offered the professional middle class an educational philosophy which enabled them to justify the effective monopoly of professional education by the existing professional class. The eugenicist could consistently advocate an expanded educational system - 1870-1914 was a period of considerable educational expansion - while laying down a structure for this expansion which maintained existing privileges. (21)

One interesting facet of the discussion of mental ability by British eugenicists is that 'business acumen' or 'entrepreneurial skills' played no part in it. We find no English Men of Business paralleling Galton's English Men of Science, although a hereditarian account of business skills could have been constructed with equal plausibility. While the majority of British eugenicists did not attack the business community, they did not seek to legitimate it in a similar way to their legitimation of the professional middle class. There was also little attempt to legitimate the hereditary nobility. Indeed, a not uncommon target for attacks by eugenicists was the House of Lords. Following Galton's views on the detrimental effect on the race of the peerage, schemes such as for the replacement of the House of Lords by/...

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(21) See K. Pearson (1902b) for an example of this type of argument. B. Simon (1971) shows the deep and persistent role of crypto-eugenic arguments in British educational thought and policy making.

by an upper house of families of genuine eugenic worth were discussed. Arnold White, for example, pictured the aristocracy and plutocracy as degenerate and prey to hereditary ills as the result of inbreeding and marriage for wealth rather than for health and mental ability (White, 1901).

Most eugenisists stopped short of an explicit attack on the existing power structure of British society. A significant section, however, attacked the existing ruling groups as unable to administer a modern society efficiently and scientifically, and condemned capitalist society as dysgenic (i.e. anti-eugenic) in its operation. A eugenic policy, they argued, was impossible while laissez-faire capitalism demanded large supplies of unskilled labour and a permanent pool of unemployed. Among socialists who at least temporarily supported eugenics were Karl Pearson, Jane Hume Clapperton and several leaders of the Fabian Society, including Sidney Webb, George Bernard Shaw and H.G. Wells. (22)

Socialist support for a movement I have analysed as representing the interests of the professional middle class seems paradoxical. However, the main point of reference/...

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(22) Pearson is discussed in chapter four; for Clapperton, see her (1885) and Farrall (1970, 32-4); for Webb, his (1907); for Wells, Hyde (1956); for Shaw, whose 'extremism' on the subject of marriage and monogamy terrified most eugenists, see the preface to Shaw (1972).

reference for Fabian socialists, and near-Fabians such as Pearson, was not the working class but the professional middle class. As Eric Hobsbawm (1968) has shown, the social composition of the Fabian Society was 'overwhelmingly non-proletarian', with journalists, writers, university and school teachers, doctors, clergy and public officials the most common occupations of its members. There were wide political differences between the Fabians and the majority of working class socialists:

The Fabians, alone among socialist groups, opposed the formation of an independent party of labour, supported imperialism, refused to oppose the Boer War, took no interest in the traditional international and anti-war pre-occupations of the left, and their leaders took practically no part in the trade union revivals of 1889 or 1911.
(Hobsbawm, 1968, 253)

But the chief concern of the Fabians was not with the working class as the agency of social change. Fabian ideology (especially as expressed by the Webbs) pivoted round the salaried middle class:

They are the trained, impartial and scientific administrators and expert advisers who have created an alternative court of appeal to profit.
(Hobsbawm, 1968, 258)

The Fabians saw in the ethos of professionalism 'a working alternative to a system in which men worked in proportion only to their financial incentive' (Hobsbawm, 1968, 258). The professional middle class would realise that a socialist society 'really suited them just as well if not better than the capitalist' (Hobsbawm, 1968, 259).

Why/...

Why should a professional middle class ideology take a socialist form? As noted in section 3.3, and as the Fabians themselves argued, there are no necessary reasons why the interests of the professional middle class should be tied to a capitalist economic order. The rising 'meritocracy' could see their skills as necessary to any industrial society, not merely a capitalist one. There were indeed particular reasons why professionals (especially in the new, rising professions) should be hostile to laissez-faire capitalism. Laissez-faire restricted the scope for their talents and their job opportunities (for example in the lack of state support for science). It could be blamed for the relatively slow growth of, for example, new professions such as that of the industrial scientist. As Hobsbawm points out, a lot of the Fabians' socialism is merely hostility to laissez-faire, not to capitalism.

Hobsbawm (1968, 266) concludes that the history of the Fabians

... must be written not in terms of the socialist revival of the 1880's, but in terms of the middle-class reactions to the breakdown of mid-Victorian certainties, the rise of new strata, new structures, new policies, within British capitalism: as an adaptation of the British middle classes to the era of imperialism.

On this view, Fabian socialism and eugenics can be seen not as political opposites but as different (though overlapping) variants of the same adaptation. Eugenics was the kind of/...

of social reform that the Fabians liked: scientific and involving planning, state action, legislation and (no doubt) an expansion of bureaucracy. If Fabian eugenicists differed from their more conservative brethren, it was perhaps only in that they took a more fundamental and long-term attitude to the interests of the professional middle class.

Fabian socialist support for eugenics thus strengthens, rather than weakens, the evidence for the hypothesis that eugenics expressed the social interests of the professional middle class. The professional men and women in the eugenics movement were 'seeking a material improvement in their status'. This can be seen in their identification of the professional middle class as the 'fit' par excellence. It can also be seen in the use of the hereditarian theory of mental ability to claim that the divide between professional, mental labour and manual labour was not merely a social division, but the reflection of a distinction between different kinds of people. These relationships do not, however, exhaust possible connections between eugenics and the professional middle class. Two further connections can be suggested, although unlike the 'class interest' aspect of eugenics they are not explicit in eugenic propaganda, and the identification of them must therefore be tentative.

The first possible link concerns the plausibility of accounting for social and economic status in terms of individual/...

individual cognitive ability. Bowles and Gintis (1976) point out that this has been a largely untested and unexamined assumption of both hereditarians and most of their environmentalist opponents.⁽²³⁾ Certainly, the existence of a strong causal connection was quite unself-consciously assumed by British eugenisists. In part, this no doubt reflected an interest in legitimating the hierarchical division of labour. It may be, however, that the very social situation of the professional middle class, as distinct from the bourgeoisie or proletariat, helped to make this assumption natural. Because social position is achieved in the professional middle class primarily through the formal educational system, individual cognitive ability might well appear to professionals to play a much larger part in determining life chances than it would to members of other classes.⁽²⁴⁾

Secondly, eugenics was particularly relevant to the problems of 'class reproduction' faced by the professional middle class of late Victorian Britain and documented by Banks (1965). A persistent theme of eugenic propaganda/...

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(23) Their own study throws doubt on the validity of this assumption for contemporary America. They use a normalised regression model to evaluate the separate causal contributions of I.Q., years of schooling, socio-economic status of parent, etc., and find that of I.Q. surprisingly small.

(24) One might, for example, speculate that members of the bourgeoisie or aristocracy would place greater weight on non-cognitive personality traits, such as 'ambition' or 'sense of duty'.

propaganda was that these problems (the cost of education, for example) were causing deliberate restriction of family size amongst the 'fit'. Again, the eugenic proposals for alleviating this problem (alteration of the incidence of the income tax, etc.) can be seen as fairly straightforward manifestations of class interest. However, it is possible that there was also an element of self-reassurance present. Rosenberg (1974, 235) argues that the 'irreducible core of the hereditarian commitment' was

... the desire for assurance that one's children would somehow be endowed with the virtues required to make their lives successful - and thus to allay the guilt, the anxious striving, the ambivalence of their parents.

Eugenics, it could thus be argued, offered the professional middle class psychological comfort in the face of anxiety about 'class reproduction'.⁽²⁵⁾

3.6 Eugenics, the Residuum and Social Imperialism

Eugenics was not only a matter of raising the fertility (and status) of the professional middle class: it/...

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(25) I have not attempted to develop any social-psychological interpretations of the individuals studied in this thesis; I lack the competence to do so. However, a reading of the letters of British statisticians of this period would certainly suggest that difficulties in parent-child relations, centring around 'class reproduction', were undoubtedly present in many cases. I mention this simply in order to point out that, if the methods of 'psychohistory' are in the future employed on the subjects of this study, any psychoanalytic interpretations that are produced should possibly be seen not as opposed to class explanations, but as complementary to them.

it also involved lowering the fertility of those at the bottom of the social scale. While this aspect of it was little emphasised in Galton's early, utopian, positive eugenics, it came more and more to the fore in the period from 1880 onwards. Within Galton's own work negative eugenics became more prominent, though he always treated the subject with a certain caution, even distaste, and avoided 'unmentionable' topics such as sterilisation and contraception. More generally, the 'unfit' rather than the 'fit' were the central focus of eugenic propaganda. What, we must ask, were the views on class structure held by the eugenisists, and who were the unfit who were to be dissuaded from breeding?

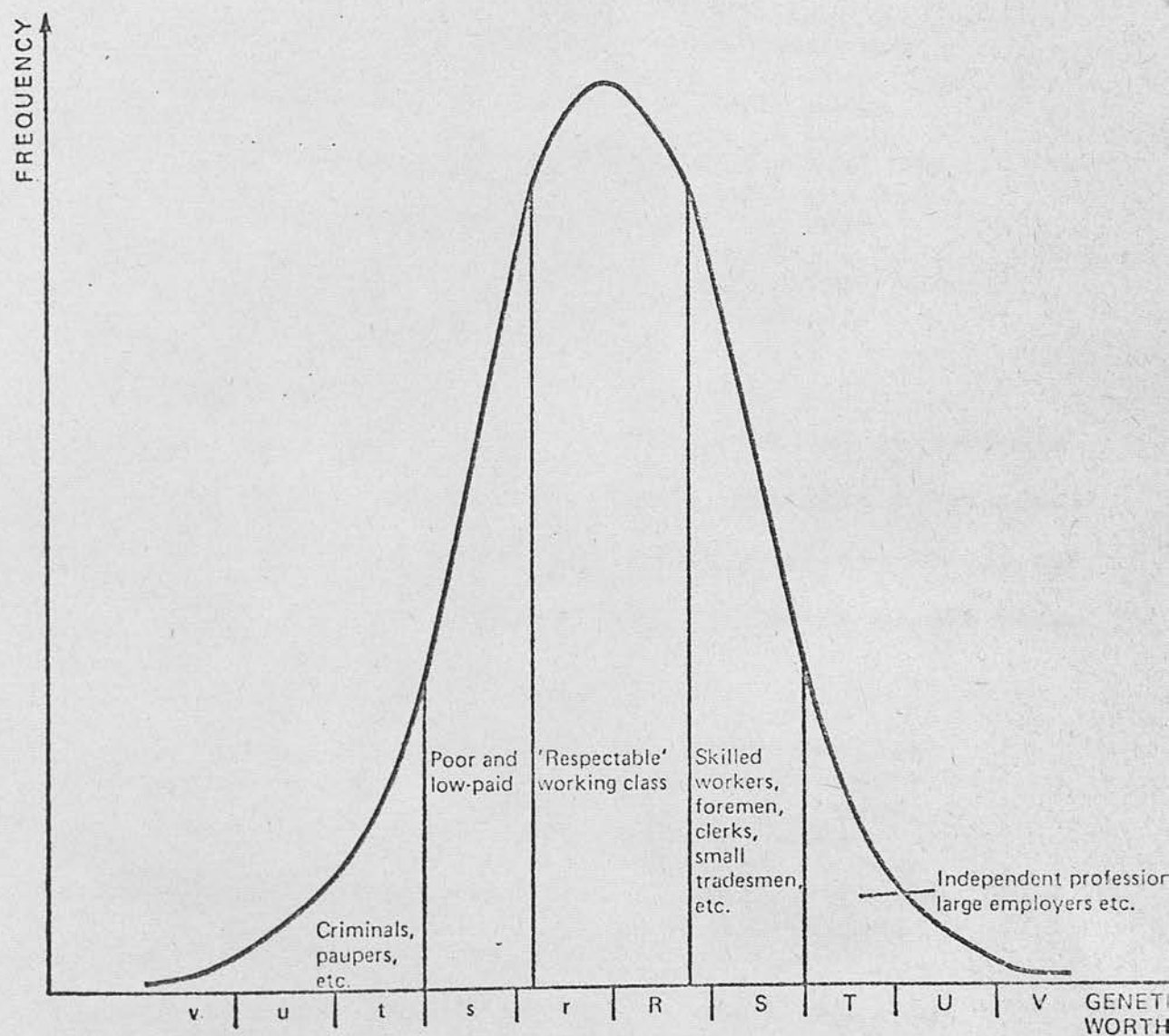
The eugenists accepted a rough equation of social standing and genetic worth. Indeed, this was generally an axiom of their thought, and seldom a proposition they felt any need to defend. At least for those social groups conventionally regarded as being below the professional middle class, class position was taken as a sure indicator of average mental ability. The view of social structure the eugenists held was summarised by Galton in his 1901 Huxley Lecture (Galton, 1909, 1-34). Galton took the social categories of Booth's survey of London and mapped them onto his assumed distribution of inherited 'civic worth'. In figure one, I have presented his results in graphical form. 'R,S,T,U,V' and 'r,s,t,u,v' are the subdivisions of 'civic worth'. The lowest group, classes, t,u,v and below/...

below, 'are undesirables' (1909, 11). It is against them (and particularly against the 'criminals, semi-criminals and loafers' of v and below) that negative eugenics should be practised; for example, habitual criminals should be 'segregated under merciful surveillance and peremptorily denied opportunities for producing offspring' (Galton, 1909, 20). Galton (and the other eugenisists) did not wish to depress the birth rate of all groups below the middle class. It would scarcely have been in the interests of the middle class to do so: eugenisists were agreed that manual workers were socially necessary. What they wanted was to improve the discipline, physique and intelligence of the working class by eradicating the 'lowest' elements of it. The eugenisists attempted to draw a line between socially useful and socially dangerous elements of the lower orders. While the exact placing of this line was vague, and varied from one writer to another, all were agreed that this distinction was necessary.

The lowest social group ('t,u,v and below') were a prominent - indeed the prominent - social problem in the eyes of middle class late Victorians and Edwardians. The attitudes of the middle class to this group have been elucidated by Gareth Stedman Jones (1971). Jones argues that in the latter part of the nineteenth century there was a shift in middle class fears about social stability. Attention was no longer focused on the heartlands of the industrial revolution (such as Manchester), but became centred on London. Since the decline of Chartism, most middle/...

Figure One

Galton's view of British social structure



For explanation, see text.

middle class observers felt that the respectable working class of the North of England were no longer a threat or a social problem. The problem rather lay with a smaller and more specific group in the slums of the big cities.

The most characteristic image of the working class was that of increasingly prosperous and cohesive communities bound together by the chapel, the friendly society, and the co-op. Pitted against the dominant climate of moral and material improvement however was a minority of the still unregenerate poor: those who had turned their backs on progress, or had been rejected by it. This group was variously referred to as 'the dangerous class', the casual poor or most characteristically, as 'the residuum'.

(Jones, 1971, 10-11)

In other words, the perceived problem of social control was no longer the working class as a whole, but only a 'residual' section of it. The largest concentration of the 'residuum' was in London. The Quarterly Review summed up middle class attitudes as early as 1855:

... the most remarkable feature of London life is a class decidedly lower in the social scale than the labourer, and numerically very large, though the population returns do not number them among the inhabitants of the kingdom, who derive their living from the streets ... for the most part their utmost efforts do little more than maintain them in a state of chronic starvation ... very many have besides their acknowledged calling, another in the background in direct violation of the eighth commandment; and thus by gradations imperceptibly darkening as we advance, we arrive at the classes who are at open war with society, and professedly live by the produce of depredation or the wages of infamy.

(quoted by Jones, 1971, 12)

The worst situation was in the East End.

From/...

From the end of the 1860's to the First World War, the East End was a by-word for chronic and hopeless poverty, and endemic economic malaise.

(Jones, 1971, 99)

There was thus a definite 'social problem' in London. The 'residuum' were not, it is true, radical or revolutionary. They were, however, politically volatile, and, pressed by extreme hardship, they were liable to riot.

Social control was not the only problem. Middle class observers felt that the poor were not only dangerous but also physically and mentally degenerate. The urban slum dweller was characteristically compared with the healthy and strong agricultural labourer. It was widely believed that urban conditions caused the degeneration of immigrants from the country, whether by the direct effect of environment or by the selection of the worst types.

Francis Galton was an early proponent of the theory of urban degeneracy:

It is perfectly distressing to me to witness the draggled, drudged, mean look of the mass of individuals, especially of the women, that one meets in the streets of London and other purely English towns. The conditions of their life seem too hard for their constitutions, and to be crushing them into degeneracy.

(Galton, 1869, 340)

Increasingly the problem of urban degeneration was seen in the context of imperialism. A degenerating population was serious enough under any circumstances, but it could be fatal to a British Empire faced with increasing foreign economic/...

economic competition, colonial war and the ultimate threat of inter-imperialist war. The early reverses suffered by British troops in the Boer War (1899-1902) gave concrete form to these misgivings. It was put about, and widely believed, that up to 60% of working class volunteers for the army had had to be rejected because they failed to meet the army's minimum standards of physical fitness (Gilbert, 1966, 84-91).

The problem, then, was seen to be a section of the working class that lacked moral fibre (i.e. was outside social control) and was physically unfit. The growth of large cities had broken the older forms of social control based on direct personal contact between rich and poor. The most important early attempt at a solution was the Charity Organisation Society, set up in 1869, which sought to reimpose social control through organised, selective charity and trained social workers (Jones, 1971, 241-61). With the deepening urban crisis of the 1880's and the serious rioting of 1886-7, there was a conscious search for new responses to the problem. Crucial to these was the distinction between the respectable working class and the residuum: the residuum must be isolated from the working class as a whole (even at the price of concessions to the bulk of workers) and neutralised or eliminated. Fabians, Tories and Liberal Imperialists could find common ground in agreement that a solution to the problem of the urban residuum was a prerequisite of imperial survival. The basis/...

basis was thus laid for social imperialism. This linking of imperialism and social reform loomed large in British politics between the 1880's and 1914, and, as Farrall (1970) points out, provided a favourable context for eugenic schemes.⁽²⁶⁾

The eugenists had a biological explanation of the 'residuum'. The suspension of natural selection through the operation of charity, medical science and sanitary reform had led, they claimed, to the flourishing, in the hearts of the great cities, of a group of people tainted by hereditary defect. Members of this group were unemployed because they lacked the health, ability and strength of will to work. Hereditary weakness turned them towards crime and alcohol. Their constitutions inclined them to wasting diseases such as tuberculosis. The 'residuum' was outbreeding skilled workers and the professional middle class. The eugenists warned that although natural selection was largely suspended within British society, competition between different nations went on. Britain was engaged in a struggle for survival that was normally commercial but might at any time become military. National fitness for this struggle was necessary. Under the conditions of modern civilisation, a replacement for natural selection had to be found in conscious eugenic selection. A pliable and fit working class could be bred by isolating the residuum in institutions where/...

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(26) See also Semmel (1960) and Searle (1971; 1976).

where parenthood would be made impossible.⁽²⁷⁾

Negative eugenics was thus not an abstract moral reform programme, but a specific response to a specific problem. The eugenicists proposed the most thorough solution to the problem of the residuum, short of immediate elimination. Social control was to be imposed by the detention in institutions of the habitual criminal, the alcoholic, the 'hereditary' pauper, and so on. Prevention of parenthood in these institutions would mean the eventual disappearance of the residuum as a group. This solution would leave untouched the position and privileges of the higher social classes, while drawing in full on the skills of the middle class scientific expert. While it might seem a rather extreme proposal, it differed only in thoroughness and scientific rationale from similar proposals put forward at the time, such as Charles Booth's plan for labour camps for the 'residuum' (Jones, 1971, 305-8).

3.7 The Rise and Decline of Eugenics

The rise and decline of the eugenics movement in Britain seems to be largely accounted for by variations in the credibility of the programme for negative eugenics. Four major turning points can be identified: the urban crisis/...

(27) This argument was made, for example, by White (1901) and by Karl Pearson (1909c; 1909d; 1909e).

crisis of the 1880's, the Boer War (1899-1902), the First World War, and the world slump and the emergence of German facism (1929-34).

Before 1880, it is impossible to talk of eugenics as a movement: it was more a utopian speculation. The urban crisis of the 1880's, and the related emergence of social imperialism, provided the context for serious consideration of negative eugenics (White, 1895; first published in 1886). The real opportunity for the eugenicists came with the Boer War and the boost it gave to social imperialism. This prompted Karl Pearson and Arnold White to write their most famous social imperialist and eugenic tracts (Pearson, 1901a; White, 1901). As White (1901, xiii) wrote:

In South Africa we have a lesson. Shall we profit by it sufficiently to reconsider our ways?

Pearson wrote to Galton urging him to open a direct campaign for eugenics, sensing that the time was ripe for 'a word in season' on eugenics (Pearson, 1914-30, 3A, 242-3). Although almost in his eighties, Galton responded by campaigning for and funding eugenics. The years from 1901 to 1914 were of gradual, if unspectacular, success for the eugenics movement, which by the time of the outbreak of war seemed on the threshold of at least some legislative impact. Prominent political figures had shown interest in eugenics, as was witnessed by the presence of names such as A.J. Balfour and Winston Churchill on the list of vice-presidents of the International/...

International Eugenics Congress, held in London in 1912. A small but growing group of M.P.'s responded to eugenic ideas, and the Eugenics Education Society was able to claim the formulation and passing of the Mental Deficiency Act of 1913 as in part the result of its work (E.E.S., 1914, 5-6).

After 1918, all this impetus had gone.⁽²⁸⁾ There was no disastrous immediate decline of British eugenics. The cadre of the movement remained intact. But eugenics seemed to lack political credibility. The E.E.S. (re-named simply the Eugenics Society) evolved gradually into a learned society rather than a campaigning political group. The broad spectrum of political support in the professional middle class evaporated. Increasingly, eugenics as a full-scale political programme became identified with the extreme right-wing. What went wrong for the eugenists?

One answer might be that the conditions for the credibility of the social programme of negative eugenics no longer existed after 1918. Before the War, the problem of social control was seen as centred on a relatively small and well-defined subgroup of the working class. After 1918, things were different. Red Clydeside and the industrial battles of the 1920's suggested that there was a pressing danger/...

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(28) Searle (1977) points out that the eugenics movement did enjoy something of a revival during the slump of 1929 onwards.

danger to established society from the working class as a whole, Unemployment was no longer localised (indeed London, the core of unemployment before 1914, was relatively prosperous during the 1920's and 1930's by comparison with the industrial North). A political strategy for the British ruling class clearly had to involve a reckoning with the working class as a whole. Such a strategy did evolve, empirically rather than theoretically, in the 1920's. Although it involved intransigence at certain key moments (notably the General Strike of 1926), the key to the strategy was an accommodation with the political and industrial leadership of the working class in the Labour Party and trade unions. This left no place for eugenics; to make the point starkly, sterilisation of the unemployed (as advocated by E.W. MacBride (Werskey, 1969)) was out of place in such a strategy. It was impossible both to reach a compromise with the official leadership of the working class and to threaten that class (or a significant subsection of it) with negative eugenics.

Most eugenisists gradually came to terms with this reality and diluted their proposals accordingly. Some, like R.A. Fisher, ceased to propagandise for eugenics, while continuing privately to hold eugenic beliefs.⁽²⁹⁾ A few maintained the old attitudes intact, and looked to the/...

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(29) Interviews with students and colleagues of Fisher have convinced me of this point.

the application of eugenic measures in the context of the destruction of the labour movement rather than that of accommodation with it. Thus, George Pitt-Rivers, formerly Secretary of the International Federation of Eugenic Societies, joined the British Union of Fascists (Werskey, 1972b, 232) and was interned during the Second World War. The Nazi victory in Germany and the subsequent Nazi eugenic measures strengthened the association of eugenics and the extreme right. After some initial hesitation, the Eugenics Society condemned Nazi eugenics. But British eugenicists found it difficult to make it clear that what they preached was different from what the Nazis practised. By the late 1930's eugenics in the old, strong, sense was identified with fascism. In the absence of gains for fascism within British society, eugenics was bound to decline.

3.8 Opponents of Eugenics

Even at the peak of its influence in the Edwardian period, eugenics was not unopposed. Within the professional middle class itself, eugenics had its critics. Clerics, particularly Catholic clerics, were notable among them.⁽³⁰⁾ These professionals of the old order had their own strategy for dealing with problems of poverty, unemployment/...

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(30) On the relations of eugenics and religion see Inge (1909; 1921) and Peile (1909).

ment, social control and the family. Despite efforts by the E.E.S. not to offend the church, eugenics appeared as an intruder into the traditional sphere of religious authority and as a competing secular and scientific ideology.⁽³¹⁾ A great deal of the reluctance of the eugenisists to advocate the use of contraceptives and sterilisation as techniques of negative eugenics can be attributed to fear of religious condemnation.

Amongst other opponents of eugenics were those socialists who, unlike the Fabians, took the working class as their prime reference group. Stella Browne, a socialist and feminist, attacked the E.E.S. for 'class bias and sex bias', and argued that women themselves should have control over their own fertility (Rowbotham, 1973, 152). Other socialists concentrated on defending the working class against the charge of genetic inferiority. In a series of articles in The New Age, M.D. Eder (1908) took the eugenists to task for their view that the 'upper middle class' represented 'the brains of the nation'. Eder reminded Karl Pearson that Gauss, whose work provided the base of much of the mathematics Pearson deployed in support of eugenics, was 'the son of a bricklayer'. De Vries's mutation theory was seen by Eder as a biological justification of revolutionary socialism, which refuted the gradualism/...

(31) This attitude comes over clearly, if idiosyncratically, in G.K. Chesterton (1922), in which eugenics is condemned from a Christian, anti-scientific and anti-industrial point of view.

gradualism of evolutionary socialists such as Pearson. After the First World War, the socialist attack on eugenics began to find a small number of supporters within science. The radical scientists of the 1930's, such as Lancelot Hogben, saw the eugenics movement as a paradigm case of the anti-working class use of science, and the defeat of eugenic ideology became one of their major preoccupations (Werskey, 1972a).

Aside from these two major sources of opposition to eugenics, particular individuals and small groups were hostile to eugenics for less general reasons. The eugenists presented their major opponent as a social reformer who ascribed all to environment and nothing to heredity. Such a parody creature scarcely existed.⁽³²⁾ Nonetheless, some groups felt their schemes for particular reforms threatened by eugenic ideology. Karl Pearson, for example, earned the wrath of temperance workers for his denial that environmental reform (temperance measures) would have a beneficial effect on the next generation (Farrall, 1970, 250-82). Similarly, Pearson's view that the major factor in the incidence of tuberculosis was an inherited tubercular 'diathesis' led to controversy with public health workers and other medical men seeking environmental control of the disease.

(32) The nearest approach is perhaps L.T. Hobhouse, who attacked eugenics from the point of view of an activist, reforming liberalism, arguing that progress was ethical and social, rather than racial. But even he accepted particular eugenic measures such as 'control of the feeble-minded'. See Hobhouse (1911).

Chapter Four

Karl Pearson

In a dialectical conception the individual ceases to be an atom which exists in isolation and opposition to other men and to the physical world, and the 'collective consciousness' ceases to be a static entity which stands above and outside particular individuals. The collective consciousness exists only in and through individual consciousness, but it is not simply made up of the sum of these. In fact, the term 'collective consciousness' is not a very satisfactory one, and I myself prefer that of 'group consciousness', accompanied in each case, as far as that is possible, by the description of the group in question: family, professional, national, class. This group consciousness is the tendency common to the feelings, aspirations and ideas of the members of a particular social class; a tendency which is developed as a result of a particular social and economic situation, and which then gives rise to a set of activities performed by the real or potential community constituted by this social class. The awareness of this tendency varies from one person to another, and reaches its height only in certain exceptional individuals or, as far as the majority of the group is concerned, in certain privileged situations: war in the case of national group consciousness, revolution for class consciousness, etc. It follows from this that exceptional individuals can give a better and more accurate expression to the collective consciousness than the other members of the group ...
(Goldmann, 1964, 18)

The aim of this chapter is to suggest the outlines of a sociological account of the thought of Karl Pearson. In many ways, Pearson is the key figure in the developments discussed here: he continued the line of work begun by Galton, notably in its statistical aspects; he made many important/...

important contributions of his own to statistical theory; and he played the central role in the early institutional development of mathematical statistics in Britain. The account developed in this chapter will be used in chapters six and seven in an attempt to explain the two major controversies in the pre-1914 history of British statistics and statistical biology.

4.1 Some Problems of Historiography

Although a fully comprehensive biography of Karl Pearson has yet to appear, some extremely valuable work on him is available. Several biographical accounts have been written, of which the longest and most comprehensive is that by his son, E.S. Pearson (1936-38), although those by George Udny Yule (1936) and Churchill Eisenhart (1974) are also useful; Eisenhart's article includes a review of the major secondary sources on Karl Pearson's work. Any study of Pearson is much aided by Morant's almost comprehensive 648 item bibliography of Pearson's writings (Morant, 1939).⁽¹⁾ Pearson as a social thinker has been discussed perceptively by Bernard Semmel (1958; 1960, 35-52) and by Lyndsay Farrall (1970). Finally, the recently completed work of Bernard Norton (1978) is closest to the approach taken here, and several of Norton's insights are drawn/...

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(1) Three additional items are listed by Eisenhart (1974, 467).

drawn on below.

From these accounts, the main features of Karl Pearson's life and work emerge clearly and are not disputed by any of the above authors.⁽²⁾ He was born in 1857. On his father's side his ancestors were Yorkshire farmers and Quakers, while his mother's family included master-mariners from Hull. Despite relatively humble origins, his barrister father, William Pearson, rose to become a moderately successful London lawyer (although at the cost of 15 hour-long working days).⁽³⁾ Karl Pearson's secondary education was received mainly at University College School, thus beginning an association with University College, London which was to last throughout his life. (In anticipation of what follows, it should be noted that University College during the nineteenth century had a decided radical and free-thinking tone compared with Oxford and Cambridge and its London 'sister', King's College.) In 1875 the young Pearson won an open-entrance mathematical scholarship to King's College, Cambridge. In 1879 he sat the Mathematical Tripos examination, emerging as Third Wrangler, and subsequently he received a College Fellowship. While holding it, he trained for the legal profession (he was/...

(2) What follows is drawn chiefly from E.S. Pearson (1936-8).

(3) Karl Pearson described him as

... an iron man with boundless working powers, who never asked a favour in his life, and never really got on because he forgot to respect any man's prejudices, and never knew when he was beaten. (Pearson to Galton, 16 October 1907. K. Pearson, 1914-30, 3A, 328.)

was called to the Bar in 1881), but also pursued more general historical and philosophical studies, spending a considerable amount of time in Germany. He did not abandon his interest in mathematics, and in 1884, he was appointed Professor of Applied Mathematics and Mechanics at University College, London (he had already published four papers in traditional areas of applied mathematics). He remained in this position until 1911, when he was appointed to the newly created Galton Professorship of Eugenics in the University of London. He held this post until his retirement 22 years later, and he continued to work at University College almost up to his death in 1936.

Pearson's published work was extraordinarily wide ranging. In the period 1879 to 1892, aside from his publications in applied mathematics, he wrote on such subjects as German history, art and folklore, philosophy, politics and social problems. His work in these areas formed the basis for his two major books of this period: The Ethic of Freethought (1888) and his famous Grammar of Science (1892a). From 1892 onwards, his attention increasingly shifted to statistics and its application to biology. In 1894 he began to teach the advanced theory of statistics and there appeared the first of a long series of 'Mathematical Contributions to the Theory of Evolution'. The collection of essays, The Chances of Death and other Studies in Evolution (1897), clearly shows this transition. Pearson soon started publishing fundamental work in the theory of statistics/...

statistics: in 1896 he introduced the product-moment formula for the coefficient of correlation and developed much of the theory of multiple correlation and regression; in 1898 he published a general theory of the probable errors and correlation of errors of frequency constants; in 1900 he introduced the tetrachoric coefficient of correlation and the chi square test. In 1900 he was instrumental in the foundation of Biometrika, the first journal to be largely devoted to the publication of statistical theory. He founded a Biometric Laboratory at University College, where much research and teaching of statistical theory was done, and also took over the running of the Eugenics Laboratory established by Galton (after 1911 directing them jointly as the Department of Applied Statistics). The publications of these two laboratories, together with the papers written by Pearson and his pupils for Biometrika, form a massive body of theoretical and applied work in statistics, of a nature and a bulk previously unprecedented. This work ranged from abstract studies of the theory of correlation to the controversial Studies in National Deterioration and Questions of the Day and the Fray. Temporarily interrupted by the 1914-18 War (when most of the resources of the Department of Applied Statistics were devoted to work for the war effort), this mammoth output continued only slightly reduced after 1918 (indeed in 1926 a new journal Annals of Eugenics was set up by Pearson to remove some of the load from Biometrika). While the volume/...

volume of Pearson's socio-political polemic was reduced, the monumental three volume Life, Letters and Labours of Francis Galton (1914-30) easily filled its place. At the same time, the Department of Applied Statistics continued as the foremost centre for advanced teaching in statistical theory.

To the historian of mathematical statistics, Karl Pearson presents something of a problem. How does one deal with a man whose interests and publications ranged from German history to the theory of evolution, from the matriarchy theory to statistical inference? The problem is that Pearson saw no watertight components within his thought: his statistical theory was explicitly published as a contribution to the study of evolution; Darwinian evolution was the theoretical basis for his political, social and ethical views. His eugenics and his statistics he saw as mutually reinforcing: at the end of his life he fought bitterly against the division of his Department of Applied Statistics into a Department of Statistics and a Department of Eugenics. We now see him as a great statistician and a somewhat less important philosopher; a contemporary such as Hobson (1905) could see him as a leading scientific apologist for imperialism. Of course, it is possible to separate out artificially one aspect of his thought and to treat that in isolation; but to their credit few, if any, of the above authors seek to do that. The various aspects of Pearson's work were far from independent/...

dependent of each other, and to study any particular aspect in isolation from the others would be to give a one-sided view, and to miss the integrated nature of Pearson's thought.

To give an adequate account of Pearson's work, it is thus necessary to study as an interconnected whole his politics, his philosophy, his eugenics, his biology and his mathematical statistics. Studies that elucidate connections between different areas of Pearson's work are therefore of obvious importance: Norton (1978), which traces the influence of Pearson's philosophy and his social Darwinism on his statistics, is the best example of this approach. Nevertheless, this kind of study - showing how Pearson's ideas on one topic can be related to his prior ideas on another - must, useful as it is, leave some questions unanswered. The first of these concerns the start of the 'chain' of interconnected ideas: the process of showing the intellectual antecedents of each phase of Pearson's work cannot be endless, yet how is it to end? The second concerns the nature of the links in the chain, of the interconnections. It seems difficult to imagine that a person's ideas at time t_2 are logically determined by those he or she had at time t_1 : people frequently discard or modify their previous beliefs. But if the connections are not those of logical necessity, what are they?

One possible solution, at least to the first of the above problems, would be to seek to ground Pearson's ideas/...

ideas in his social background and early experience. But this would seem an unpromising research programme. There were many children from families of upwardly-mobile professionals who developed views quite dissimilar to Pearson's, and therefore in what sense can it be said that Pearson's background was the cause of his beliefs? If we had complete knowledge of his background and early experiences, then it might be possible to isolate those factors which made Pearson develop one set of views, while others from a broadly similar background developed different views. However, this approach is no more hopeful: in part because it calls for data we do not possess, in part because it presupposes a view of the efficacy of socialisation which does not take into account the fact that individuals frequently change their ideas and affiliations as they move through life. A deterministic account of an individual's thought thus runs into insuperable theoretical and practical difficulties. But is it not possible to produce a sociology of knowledge account of an individual's thought, which does not involve the attempt to see that particular individual as socially determined?

4.2 The Sociology of Knowledge and Individual Actors

The approach I will take is, broadly, that suggested by Lucien Goldmann, and outlined in the quotation at the head of this chapter. This approach, in my opinion, provides/...

provides one possible solution to a major problem of the sociology of knowledge. In the sociology of knowledge, at least as classically conceived, explanation is structural in its nature. Particular ideas, or sets of ideas, are related systematically to social-structural categories. This is done, broadly speaking, in two ways. Ideas are seen as reflecting the social interests of actors in a particular social-structural category (for example, the interests of the bourgeoisie) or as embodying the social experience of such actors (for example, the class experience of the proletariat). The analysis of eugenics in chapter three is an example of this kind of imputation. However, as Goldmann points out, it is the individual, not the structural category, who is the bearer of concrete ideas. If the sociology of knowledge is not to be a purely theoretical endeavour we must therefore study individuals or groups of individuals, and in the sociology of scientific knowledge, these groups are likely to be quite small. The problem thus arises of how the structural explanations of the sociology of knowledge relate to the study of concrete individuals.

A crude form of the sociology of knowledge dissolves this problem by the implicit assumption that all actors belonging to a particular structural category⁽⁴⁾ will think/...

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(4) In what follows 'class' will be taken as typical of a structural category, but it is not implied by this that 'class' is the only category to which imputations can be made.

think in the manner held to be appropriate to that category. This, indeed, is the version of the sociology of knowledge frequently put forward by those seeking easy refutations of the sociality of thought. It is manifestly false. A somewhat less crude view of the sociology of knowledge weakens this assumption by making it a statistical one: we must expect a majority of actors to think in the appropriate fashion. This is a view well adapted to the situation of those who perform attitude surveys, but in historical work we are seldom in a position to test hypotheses in this form. In any case, it is hard to see why any particular proportion of individuals should be crucial. Why 50%, and not 60%, or 20%? The empiricist formulation of this kind of approach will tend to leave it unable to explain, except in an extremely ad hoc fashion, the sudden changes in consciousness often observed, in particular amongst subordinate classes, in crisis situations.

It is possible, however, to solve this problem in a way that avoids these pitfalls without abandoning empirical reference. The solution hinges around seeing social-structural explanations as accounts of tendencies. That is, in essence, the solution proposed by Goldmann (1964), and before him by Lukács (1971; first published in 1923). The interests and experience of a particular class give rise to specific tendencies in the thought of that class. These tendencies are not psychological biases or dispositions: they are tendencies associated with sets of roles/...

roles, not necessarily with particular individuals. No denial is implied of the individual's freedom to construct idiosyncratic idea systems for himself or herself. Nor are these tendencies necessarily manifest. We can say, correctly, that 'glass has a tendency to break', even if no window pane is ever broken: we cannot prove this proposition true or false by a statistical survey of windows.

Similarly with classes. Tendencies in the thought of a class will not normally be unopposed. Past traditions, the thought of other cultures and other classes may be sufficiently strong to swamp any tendency to thought of a particular kind. In subaltern classes, exposed as they are to a wide variety of institutions of ideological domination, these countervailing tendencies can be expected to be particularly strong.

So this form of imputation involves no necessary predictions at the individual level, either of a universal or of a statistical nature. However, it is not without empirical reference. It is true that a study done of a class at one particular time (for example, an attitude survey) is unlikely to tell us much about the truth or falsity of such structural imputations of tendencies. However, longitudinal and historical studies can be of use. As Goldmann suggests, in situations of crisis, when countervailing tendencies weaken, we should expect to see a heightening of the expression of imputed tendencies. Other things being equal, as the social coherence of a class increases, as it is more strongly differentiated from other classes/...

classes, as it finds itself in conflict with other classes, these tendencies should grow stronger.

Similarly, we should expect these tendencies to be strongly manifested in the thought of some individuals, and not in that of others. Those in whose thought imputed tendencies are particularly strongly manifested are Goldmann's 'exceptional individuals'. Goldmann, however, gives no rule for predicting in advance who these individuals will be: they are identified only ex post facto on the basis of their thought. But this leaves unclear the sense in which these individuals are to be thought of as exceptional. For Goldmann, it is in practice not the individuals but their ideas which he analyses as exceptional: because their ideas express the interests and experience of a class in its purest form, they possess an exceptional coherence and aesthetic merit. This formulation, however, is hardly satisfactory from the point of view of the sociology of knowledge, despite its possible merits as a theory of aesthetics, because it gives no account of the origins of this purity of expression and coherence.

There is, however, another way to develop Goldmann's notion. All societies of any complexity are structured in more than one way and at more than one level. Thus we can identify within any given society an overall structure, such as a class structure, and a fine structure, consisting of all sorts of more particular gender, occupational, kinship or generational/...

generational structures, and of specific institutions such as state apparatuses, educational institutions, political parties or trade unions.⁽⁵⁾ If our theory seeks to relate ideas to the overall class structure, then we must expect the fine structure of the society, insofar as it does not run parallel to the overall structure, to generate particular interests and experiences and thus to cross-cut and 'suppress' this relation. The fine structure produces 'noise' from the point of view of our overall pattern of explanation. So perhaps we can expect 'exceptional individuals' to be found in structural locations and historical situations where the 'distorting' effects of the fine structure are least. It is clearly impossible without much study to specify these locations and situations. One tentative suggestion is that individuals in marginal roles, or those who have moved from one class to another, could be particularly sensitive to the overall rather than the fine structure.⁽⁶⁾ But this clearly needs much refinement before it can become a usable theory; and in any case structural accounts do not depend for their validity on this type of conjecture.

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- (5) Of course, what is seen as the overall structure is dependent on the theoretical perspective employed. One might, for example, see gender structuring as primary, and class structuring as a form of fine structure overlaying this.
- (6) Compare the role of marginal individuals in scientific and technological innovation as discussed, for example, by Edge and Mulkay (1975).

Even if exceptional individuals can, at the present, only be identified ex post facto, that does not imply that analyses of them need be uninformative. It is clear that the study of these individuals cannot, because of the way they are selected, provide an independent check on the validity of our theory. But, if our theory is correct, we should at least expect it to provide a coherent and convincing account of the thought of these individuals. Further, we should expect that if the ideas of these individuals be available to other members of their class at a time when, in the sense suggested above, their class consciousness is (on our theory) high, then these ideas should be well received. Given necessary conditions - such as the physical dissemination of their ideas - the thought of these exceptional individuals should in some sense be successful.

To proceed with this approach, we first need a theory of social structure which is applicable to the historical situation in which we are interested. Ideally, this theory should be precise, well-developed and with a record of successful application in fields other than the sociology of knowledge. From this theory, together with an understanding of the historical situation, the next stage is to produce an account of the interests and social experience of the class (or other social-structural category) in question. This might include an account of its economic interests, its relationships of competition or co-operation with/...

with other classes, salient work or other experiences of its members, and so on. Then the field of knowledge in which we are interested should be examined, with a view to determining key features of the pre-existing system of knowledge, and the relationship of that knowledge to social practice and to the interests of other social classes. Knowledge is never constructed ex nihilo, and thus the pre-existing state of knowledge is always an important factor in determining how those who construct new knowledge in fact do so. From our theoretical account of the interests and experience of the social class in which we are interested, together with our account of the situation in which it finds itself, in particular as regards pre-existing systems of knowledge, it should then be possible to posit tendencies in the thought of this class and in the society generally. This account of the tendencies of thought can then be put to use in ways suggested schematically above. The account will stand or fall on the basis of its success in explaining past developments of thought or in predicting future developments. (7)

Chapter three can be taken as an attempt to begin this/...

- (7) Perhaps the most interesting case, and certainly the most important one, is where the theory in question is used as a guide to political practice. Thus Marxists posit a 'tendency' in the thought of the working class, and part of Marxist political practice is to attempt to enhance that posited tendency in ways suggested by Marxist theory. The success of this forms a 'criterion of practice'. This is perhaps the closest we can get in the sociology of knowledge to experiment in the natural sciences.

this type of analysis for the British professional middle class.⁽⁸⁾ A particular social-structural category - the professional middle class - was identified, and, by examination of its structural and historical situation, certain tendencies in its thought were predicted. These included tendencies such as that to the emphasis on accredited knowledge and expertise as the proper basis for social status. Eugenics was described as a system of belief which exemplified these tendencies; Fabianism was taken as another example. The aim of this present chapter is to go beyond this general analysis, and to investigate the extent to which a Goldmann-style 'exceptional individual' analysis can be applied to Karl Pearson as a member of this class.

4.3 Pearson's Politics

Karl Pearson thought of himself, and was thought of by his contemporaries, as a socialist. This fact has in the past been uncomfortable to those who wish to fit Pearson into neat categories. Thus, in his investigation of the connections between hereditarianism and conservatism, on the one hand, and environmentalism and liberalism or radicalism, on the other, Pastore (1949, 29-41) sees Pearson as/...

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(8) In real historical research, the analysis can never be as mechanical as the schematic outline given in this section might suggest. As Goldmann argues, it is necessary to move dialectically between the society and the body of beliefs in question: each illuminates the other.

as a socialist environmentalist before 1900, but a conservative hereditarian after 1900. As against this, it will be argued below that there was a continuity in Pearson's views throughout his life; that he became a hereditarian well before 1900; and that his politics can indeed be described, within the Victorian understanding of the term, as socialist. There was no contradiction between his variant of socialism and his hereditarianism: in fact, the relationship between these two aspects of his beliefs is very important in helping us understand his thought.

As Bernard Norton has noted, Pearson was a 'non-joiner'. Although the most important eugenist after Galton, he did not join the Eugenics Education Society; although the most prominent British statistician, he did not join the Royal Statistical Society. He did join the Royal Society of London (if the offer was made one could hardly refuse), but subsequently spent a considerable time on the brink of resignation. He was a seeker after perfection: the flaws inevitable in any real-world institution irritated him and led him to loathe committees and societies. Another society he did not join was the Fabian Society; this was despite the fact that he was, in a sense to be elucidated below, the most consistent Fabian. Among Pearson's personal acquaintances were leading Fabians such as Sidney Webb and George Bernard Shaw; Pearson wrote a generally favourable review of the first edition of Fabian Essays (Shaw (ed.), 1889; Pearson, 1890); in the introduction/...

introduction to the second edition of his Ethic of Free-thought, Pearson complimented the Fabians on their 'excellent educational work' in bringing socialism to the forefront of public debate (1901d, vii). On the other hand, Pearson had no time for the more radical alternative to the Fabians, the Social Democratic Federation (Pearson, 1888, 7).

The elements of Pearson's political position were in fact worked out before the Fabian Society was founded in 1884. In 1879, on one of his early trips to Germany, Pearson met Raphael Wertheimer, a Jewish law student and socialist (Norton, 1978). Wertheimer appears in the fictionalised letters of Pearson's first book, The New Werther (1880). Pearson was at this time particularly open to new and radical ideas. His career at Cambridge had been marked by battles with authority over compulsory divinity lectures and chapel. Despite his academic success and the friends Cambridge had brought him, he left somewhat disillusioned and in search for the creed he had failed to find at Cambridge:

'Tis coin or custom draws men to this spot,
Or, since it was their father's lot,
Or, to gain social stamp, but not to learn;
While teachers only teach to earn.
(Pearson, 1881a, 191)

Wertheimer introduced the young Pearson to the ideas of the 'Socialists of the Chair' and of the emerging reformist wing of German social-democracy.⁽⁹⁾ The 'Socialists of the Chair/...

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(9) This confirms Bernard Semmel's speculation (1958, 113) that Pearson came into contact with the ideas of the 'Socialists of the Chair' while in Germany. For some of the ideas of the Katheder-Sozialisten see Schumpeter (1954, 800-20).

Chair', as portrayed by the fictionalised Wertheimer in The New Werther, did not seek revolution, but rather a slow and progressive state take-over of large enterprises, factories and the land. They appreciated that while change was necessary, all real change must be gradual (Pearson, 1880, 34). This essentially Fabian doctrine Pearson picked up and made his own, and he integrated it with a perspective on British social structure and the situation of British society.

The earliest clear expression of Pearson's emerging political position is in an article he wrote for the Cambridge Review entitled 'Anarchy' (Pearson, 1881b). Pearson's subject was revolution. He examined the state of the European socialist movement, seeing it as essentially split between the followers of 'democratic socialism' on the one hand, and 'revolutionary anarchy' on the other. Marx, he saw as supporting the former wing in theory, but the latter in practice:

... Marx as theorist is one of the most powerful, logical, and sharpest of thinkers, though as practical politician it is hard not to condemn him severely.
(Pearson, 1881b, 269)

Pearson claimed that, up to the end of the 1870's, the 'democratic socialists' had been dominant, and he cited as an example the defeat of Bakunin by Marx within the Communist International. However, he felt the pendulum had then swung towards the 'revolutionary anarchists'. The attempts by Hödel and Nobiling to assassinate the Kaiser and/...

and the passing of the Anti-Socialist Law had thrown the German socialists 'entirely into the hands of the anarchical party' (1881b, 269). The socialist movement had returned to a revolutionary perspective:

In the early part of this year, the largest meeting of Internationalists ever held assembled in London; the German Socialists were represented by Marx, Liebknecht, Bebel, etc., and the Russian Nihilists were also represented. A complete reunion must have taken place between the two parties ... We may then look upon the German Socialists and the Russian Nihilists for the future as the secret party of anarchy ...

(Pearson, 1881b, 269; Pearson's emphasis)

There was, Pearson claimed, a 'party of anarchy' at work even within Britain, which, although at present probably small, was potentially very dangerous.

The revolutionary anarchists would 'probably have little if any influence on the better class of working man'. But the urban 'residuum', not the 'better class of working man', constituted the danger:

... in the dumb, helpless masses of our great towns, the Proletariat pure and simple, they [the anarchists] foster that process of fermentation which is but too surely progressing.

(Pearson, 1881b, 269)

Pearson pointed not just to the poverty and degradation of the urban masses, but also to their insurrectionary potential:

The chance visitor to London, who sees the constant flow of busy faces in the Strand, the marshalled lines of chariots in the Row, the wealth and prosperity scattered around, would scarcely believe in the anarchical element existing in strength in such a town. But let him cross Blackfriars Bridge on a Saturday night, let him penetrate into the Borough/...

Borough, let him make a Pilgrimage on Sunday to the back of Soho, and his opinions as to the stability of society may be somewhat shaken. There he will see the stunted forms, the pallid faces, deeply lined with what the Westender in ironic simplicity calls vice, the scarcely clothed, the scarcely sexed figures of the Proletariat ... Those emaciated beings, weak and feeble as they look, have power to break the half-inch of glass which separates them from the weapons they require, have power in their millions to throw down the few feet of bricks which guard the arsenals. Those three millions could sweep a few thousand police and soldiers before them as the wind blows a handful of chaff ... Again, let me repeat, the Proletariat [sic] have nought to lose but their chains, a world to gain. Anarchy can bring them no harm, they can but benefit from it ... Anarchy, and what then? Night, blackest night, and but faint chance of a dawn. The revolution which is imminent is not a second French Revolution, the triumph of the Bourgeoisie over an aristocracy of birth, but the victory of the Proletariat over an aristocracy of wealth, over that very Bourgeoisie itself. In the struggle mighty empires will fall and ancient thrones be shaken. It looms as a gaunt spectre over the closing years of the nineteenth century ...
(1881b, 269-70; Pearson's emphasis)

There was only one way, Pearson felt, that this calamity could be avoided: 'the revolution must be carried through from above'. But to rely on the bourgeoisie to do this was impossible, as they would not part with their wealth without a struggle. Further, simply removing wealth as a factor stratifying society was not sufficient: it had to be replaced by another factor.

Anarchy can only be prevented by maintaining some kind of society with its forms and grades, but if we do not graduate society on the scale of wealth, on what shall we graduate it?
(1881b, 270)

Different stages of society had been stratified, Pearson said/...

said, first according to strength, then according to a combination of strength and 'nobility of feeling', then by 'nobility of birth', and finally by wealth.

What can take the place of this? Possibly, education and culture may. So that while power material shall be divided as equally as may be between the various classes, power intellectual shall form a scale on which the necessary graduation of society may take place. Power intellectual shall determine whether the life-calling of a man is to scavenge the streets or to guide the nation.
(1881b, 270)

The article, however, ended on a gloomy note despite this suggestion. Pearson doubted whether the 'ruling Bourgeoisie' would accept such a standard of 'power intellectual'. Christian religion, which used to form 'a real bond between class and class', had ceased to be effective, and there was no indication of a new religion emerging to 'draw mankind together'.

We seem as it were drifting helplessly onward to the brink of a terrible and unexplored abyss ...
(Pearson, 1881b, 270)

In this article, together with the passages on socialism in The New Werther, the outline of Pearson's political viewpoint is already present. His image of society was of a fundamental divide between the 'ruling Bourgeoisie' on top, and of the 'proletariat pure and simple' at the bottom. Above the 'proletariat pure and simple' was 'the better class of working man'. Although Pearson was not specific at this point about what lay between the latter class and the bourgeoisie, it is clearly in that intermediate area/...

area that he placed himself. British society, as then organised, he judged to be in a state of dangerous instability. He felt that change was inevitable. He was, however, very concerned that social change should not be abrupt, revolutionary and anarchic, but gradual and guided from above. Class conflict should be avoided: indeed the socialist should preach class harmony rather than class conflict. The state should be strengthened, and citizens should be taught that their primary loyalty lay to the state, not to their own personal or class interests. Land and capital should be nationalised, but in a slow and progressive fashion. Stratification by wealth should be replaced by stratification by mental ability. This position was in his later writings to be amplified and extended: by the proposal of a specific means - the conversion of freehold in property to 100 year leasehold - for the slow take-over of property, by the development of detailed blueprints for an education system to ensure that the able were properly trained for power,⁽¹⁰⁾ and so on. Nevertheless, it remained essentially unchanged except for its articulation in an increasingly hereditarian framework.

In a paper, 'Socialism in Theory and Practice', delivered in 1884 to a working class audience (Pearson, 1888, 346-69), the contours of Pearson's view of society emerge clearly. There was a governing class in Britain, Pearson told/...

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(10) See K. Pearson (1888, 367-8; 1902b) for these schemes.

told his audience, which was composed of the 'owners of land and owners of capital'. This class 'naturally governs in its own interests'. Two classes were excluded from power by this governing class:

The educative class (the class which labours with its head) and the productive class (the class which labours with its hands) have little or no real influence in the House of Commons.
(Pearson, 1888, 348)

He called for the transition from a social system based on wealth to one based on labour. But this immediately raised the point of the relationship of mental and manual labour.

I have met with certain working-men, who believed nothing but labour of the hand could have any value; that all but labourers with the hand were idlers.
(1888, 353)

Both head work and hand work, Pearson asserted, are forms of labour:

The man who puts cargo into a ship is no more or less a labourer than the captain who directs her course across the ocean; nor is either of them more of a labourer than the mathematician or astronomer whose calculations and observations enable the captain to know which direction he shall take when he is many hundred miles from land. The shoemaker or the postman are no more labourers than the clerk who sits in a merchant's office or the judge who sits on the bench. The schoolmaster, the writer, and the actor are all true labourers.
(1888, 353)

Because all kinds of labour are necessary parts of an integrated division of labour, it must be an 'axiom' of socialism that 'all forms of labour are equally honourable'. Nevertheless, there was little doubt in Pearson's mind that/...

that head work was, in the long run, more important than hand work.

There is labour of the hand, which provides necessities for all society; there is labour of the head, which produces all we term progress, and enables any individual society to maintain its place in the battle of life - the labour which educates and organises.
(1888, 355; Pearson's emphasis)

Thus, Pearson's socialism - while it unquestionably was in the categories of the time validly describable as socialism - in no way implied a shift of identification to the working class. It was to the class of 'head workers' that he owed allegiance. Certainly Pearson was no egalitarian, nor did he interpret socialism as having any egalitarian consequences.

So far as I understand the views of the more active socialists of to-day, they fully recognise that the better posts, the more lucrative and comfortable berths, must always go to the more efficient and more productive workers, and that it is for the welfare of society that it should be so.
(Pearson, 1897, 1, 112)

The only major instance of practical political activity by Pearson is interesting in this light. This was his campaign for the absorption of the separate university colleges in London in a single university controlled by its professoriat. This involved Pearson in the establishment of an 'Association for Promoting a Professorial University for London'. It brought him into conflict with the authorities of University College, who sought a federal solution in which, no doubt, they rather than/...

than the teaching staff would have control. It even found him opposed to Thomas Henry Huxley, who was seeking a compromise solution. While this was undoubtedly political activity of a radical nature (there was talk of sacking dissident staff), it was activity clearly aimed at the furtherance of the interests of this particular group of 'head workers'. (11)

Pearson's political position was essentially Fabian. (12) Like Fabianism, it can be analysed as an ideology appropriate to the interests of a rising professional middle class. It was a strategy for containing the working class threat by a process of gradual reform, while slowly edging the bourgeoisie out of positions of power, and replacing a society based on wealth by one based on knowledge and mental skills. In its full development, Pearson's position can in a certain sense be seen as more consistent than the Fabianism of the Fabian Society. The crucial issue on which Pearson differed from the majority of Fabians was that of political democracy and the extension of the franchise. The Fabians saw universal suffrage as the path to socialism. Pearson did not. Reviewing the first edition of Fabian Essays (Shaw (ed.), 1889), Pearson wrote (1890, 198):

On/...

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(11) For an account of this campaign see K. Pearson (1906, 285-8). Pearson's writings on the issue are collected in Pearson (1892b).

(12) See Hobsbawm (1968) and chapter three above.

On the one hand, 'socialism postulates democracy', on the other 'democracy holds socialism in its womb'. These are the oft-repeated doctrines of our Fabians. Personally dreading an uneducated democracy as much as a prejudiced aristocracy, and thinking that of the two the former is the slightly less stable form of government, we cannot but deprecate this identification of socialist and social-democrat.

Uneducated people would be incapable of choosing leaders whose view of socialism was suitably gradualist; rather, they would vote for a demagogue who would offer immediate benefits at the expense of long-term social stability. Democracy might then be the 'worst foe' of socialism. By comparison with this, Pearson preferred even the 'autocratic socialism' of Bismarck, dealing out reforms with one hand while repressing the social democrats with the other. Pearson's ideal was, as he expressed it elsewhere, 'the cautious direction of social progress by the selected few' (Pearson, 1888, 322).

What are we to make of this divergence? Aside from this point, Pearson's views on socialist strategy coincided almost exactly with the Fabians'.⁽¹³⁾ It was not the case that Pearson had a more jaundiced view of the working/...

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(13) He did disagree with proposals of a guild-socialist rather than state-socialist nature (of which there were some in Fabian Essays), arguing that autonomous worker-controlled enterprises were doomed to failure. Many Fabians would, however, have agreed with him. Thus, Beatrice Webb was delighted to find that in Stalin's Soviet Union 'there is no d-d nonsense about Guild Socialism!' (quoted in S. and B. Webb, 1975, xxxvii). Pearson's other disagreements with the Fabian Essays centred on what he felt to be a failure to deal adequately with the population question and with feminism (see below).

working class than did most Fabians. In 1889 the Fabian journal Today did not merely approve Booth's plan to force the chronic poor into labour colonies, but enthused about it as a harbinger of the collectivist change Fabians desired (G.S. Jones, 1971, 314). Rather, the difference between the Fabian Society and Pearson should perhaps be seen as lying in the difference between expediency and consistency. The Fabians were seeking political influence, first through the Liberal and later the Labour Party: an extension of the franchise, they calculated, could only increase the pressure to social reform, and thus strengthen their position. The 'fine structure' of British politics dictated that they support the extension of political democracy. Critics of the Fabians indeed sensed that their commitment to democracy was less than total. 'At heart [their] principal leaders are bureaucrats not democrats', one wrote (quoted by Hobsbawm, 1968, 264). Pearson, on the other hand, was uninterested in calculations of particular political advantage. Like many Fabians, he feared and distrusted the working class, especially the urban slum dwellers: any measure that might place potential power in their hands he resisted. In this sense, he was more consistent than they were: he in his thinking was affected only by the 'overall' structure of classes; they by/...

[15] See, for example, P.M. Turner (1974).

by the fine structure of institutions.⁽¹⁴⁾

4.4 Pearson's Darwinism

As a scientific (and scientistic) intellectual in late Victorian Britain, it is not surprising that Karl Pearson should have been an ardent Darwinian. To be a Darwinian was to ally oneself with progress against reaction, with the secular against the religious, and with the rising scientifically-based professions against the still powerful Established Church. Despite the availability of a whole range of intermediate positions between Darwinian naturalism and scriptural anti-Darwinism,⁽¹⁵⁾ the potency of Darwinism as a cultural symbol remained almost undiminished. Pearson embraced that symbol ardently. Interestingly enough, however, he did not do so until the mid-1880's (after his first writings on politics, philosophy and history), and the manner in which he finally came to Darwinism is of some significance.

Pearson/...

(14) It is not that Pearson was more consistent in any absolute logical sense. Logically, it is perfectly consistent both to fear the working class and believe them inferior, and to argue that the franchise should be fully extended to them: one's aim might for example be to defuse working class protest. Perhaps it could be said that Pearson and the Fabians were following different logics: his reflecting the overall social structure, theirs a particular aspect of the structure of political institutions.

(15) See, for example, F.M. Turner (1974a).

Pearson did not come to Darwinism as a biologist (he showed almost no interest in biology as such until after 1890) nor even, primarily, as a freethinker seeking a weapon against revealed religion. To him, Darwinism was first and foremost a theory of history.⁽¹⁶⁾ His early thought about history and historical change was cast in quite a different idiom, that of German idealism and historicism. Thus, he compared 'Manchester' political economy unfavourably with the German historical school, and indicated his acceptance of Fichte's 'socialist' theory of the state (1881c, 124; Pearson's emphasis):

... Fichte, arriving at the conception that every species has its purpose, argues that if, in the development of mankind, the purpose of the species man is ever to be realised, so must all individual forces be united and directed to this one purpose. Necessarily therefore an organisation must exist which compels individuals to put all their force into this one direction. That organisation is the State.

Pearson's attitude to the state was clearly incompatible with the dominant individualistic and laissez-faire social Darwinism epitomised by Spencer. Pearson thus found it necessary to transform social Darwinism before he could become a Darwinian, before he could write that 'the philosophy of history is only possible since Darwin' (1888, 430).

The way in which he did this was simple: he argued that the chief locus of the struggle for existence was no longer/...

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(16) Bernard Norton's suggestions on this point I have found particularly useful: for some of these, see Norton(1978).

longer the individual but the group. The traditional social-Darwinist objection to socialism, that by suspending individual competition it would suspend natural selection and cause the race to deteriorate, was invalid. The spur to efficiency was not individual competition, but inter-group struggle: survival went to the fittest group, not the fittest individual. In inter-group struggle, the social organisation of the group counted for as much, or indeed more, than the individual fitnesses of the individuals comprising the group. The internal competition that resulted from laissez-faire capitalism weakened a nation in international struggle. A class-divided nation, with an unfit and disaffected proletariat, could hardly hope to compete successfully with a well-organised and united state. (17)

Pearson was by no means the only individual who, in the 1880's, was seeking to modify the individualistic thrust of previous social Darwinism. By 1889 Sidney Webb could write in Fabian Essays:

We know now that in natural selection at the stage of development where the existence of civilised mankind is at stake, the units selected from are not individuals, but societies. (Shaw (ed.), 1889, 89)

D.G. Ritchie (1889) also put forward similar arguments to Pearson about the relationship of Darwinism and socialism. It/...

(17) The fullest statement of the above views is the essay 'Socialism and Natural Selection', first published in 1894 (1897, 1, 103-139), but they can be seen in embryo in The Ethic of Freethought (1888).

It was, of course, natural that those who formed the 'socialist revival' of the 1880's should seek to show that Darwinism need not be individualist and laissez-faire in its social implications. But another factor, wider than the British socialist revival, also may have been at work: the growing consciousness of imperialism. 1880 is conventionally taken as the beginning of the 'Age of Imperialism' (Gollwitzer, 1969). Although there clearly was a consciousness of imperialism before 1880, the early 1880's saw, for example, the foundation of explicitly pro-imperialist movements such as the Primrose League (1883), the Imperial Federation League (1884) and the protectionist National Fair Trade League (1881) (Gollwitzer, 1889, 105; Browne, 1974, 56). There was an increasing awareness of the economic importance to British capitalism of overseas investment and colonies, and also of the fact that, following the establishment of the German Reich in 1871 and the rapid industrial growth of post-Civil War America, British supremacy was no longer automatic. Although it was only after 1900 that this consciousness reached its height, from the late 1880's awareness of the reality of inter-imperialist competition was a growing factor in British politics.

The connections between imperialism and the 'external' social Darwinism of the 1880's and after are intimate. The 'internal' social Darwinism of Spencer could be, and was, used to legitimate a laissez-faire, competitive, capitalist order. 'External' social Darwinism took/...

took its model of group struggle from the economic and military competition of the advanced nations and the ruthless suppression of 'inferior' peoples: at the same time it was used to legitimate imperialism. (18)

These two explanations of the transition in social Darwinism in the 1880's - the re-birth of socialism, and the growing consciousness of imperialism - should not be taken as competing. As Semmel (1960) has pointed out, there was in this period a strong connection between imperialism and movements for social reform. Social reform was necessary to secure national efficiency in the inter-imperialist struggle: social reform could be financed from the profits of imperialism. While non-socialists, such as Joseph Chamberlain, played on this connection, to the Fabians it was particularly vital. 'National efficiency' in the inter-imperialist struggle was the key slogan, the short-cut to power for the scientific expert and specialised administrator (Searle, 1971). In practice, this Fabian policy was less than totally successful, in that their support for the Liberal Imperialists such as Roseberry against Campbell-Bannerman and Lloyd-George turned out to be inexpedient (Hobsbawm, 1968, 253). Nevertheless, their diagnosis/...

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(18) Hofstadter notes a similar transition in social Darwinism in the United States (1968, 202), although it seems to take place rather later in the United States, perhaps because of the somewhat later development of expansionist tendencies in that country.

diagnosis that inter-imperialist competition was a spur to technocratic and collectivist reform was surely right, even if it took the First World War to prove it (Marwick, 1967, 162-202 and 244-276). Thus, it is important not to let the present anti-imperialist connotations of socialism cloud our analysis. Imperialism and socialism of the Fabian variety were, in the period 1880-1914, parallel rather than conflicting developments.

Pearson developed perhaps the most thoroughgoing scientific rationalisation of social-imperialism, this fusing of imperialism and social reform. Semmel (1960) takes him as his first example of a social-imperialist thinker, and, as noted above, the anti-imperialist Liberal Hobson saw him as a leading scientific defender of imperialism. From early on, Pearson was decidedly pro-imperialist. In his talk, 'Socialism in Theory and Practice', he told his audience:

Some of you may be indifferent to the great empire of England, to this superiority of Englishmen, but let me assure you that, small as in some cases is the comfort of the English working classes, it is on the average large compared with that of an inferior race - compared say with the abject condition of the Egyptian peasant.
(1888, 354)

Certainly, the tone of his imperialism became harsher as time went on. His most important social-imperialist tract, National Life from the Standpoint of Science, contained such sentiments as:

The path of progress is strewn with the wreck of nations/...

nations; traces are everywhere to be seen of the hetacombs of inferior races, and of victims who found not the narrow way to the greater perfection. Yet these dead peoples are, in very truth, the stepping-stones on which mankind has risen to the higher intellectual and deeper emotional life of today.
(1901a, 62)

But this should not be interpreted, as it is by Pastore, as abandonment of his early socialism. Previously to this, he had linked equally imperialist sentiments to socialism:

No thoughtful socialist, so far as I am aware, would object to cultivate Uganda at the expense of its present occupiers if Lancashire were starving. Only he would have this done directly and consciously, and not by way of missionaries and exploiting companies.
(1897, 1, 111; Pearson's emphasis)

Was social-imperialism, like Fabianism, an ideology of the rising professionals? Can we see Pearson's social Darwinism - which clearly reflected his social-imperialism - as the expression of his social interests as a professional in an imperialist nation? We can, but with one reservation. There is a clear sense in which social-imperialism can be seen as in the interests of the professional middle class. Imperialism as such broadened the opportunities available to members of this group:

The practical tasks which imperialist policy gave to explorers, geographers, doctors, engineers, technicians, and numerous other specialists with scientific or technical university qualifications, certainly did a great deal to win over these groups to the imperialist movement.
(Gollwitzer, 1969, 86)

The specifically 'social' side of social-imperialism - collectivist, technocratic reform - was also in the interests of this group, providing them with increased authority and/...

and job prospects. However, we cannot analyse social-imperialism as an ideology of the professional middle class alone. The interests of the bourgeoisie and, according to Lenin (1973, 107-113), those of the 'aristocracy of labour' also were served by social-imperialist policies. Social-imperialism cannot, then, be regarded as imputable to a single class in a manner similar to Fabianism. Nevertheless, the point remains that, in developing his social Darwinism in the way he did, Pearson was formulating an ideology expressive of the interests of his class in its historical situation.

A further aspect of Pearson's Darwinism needs discussing, although it does not represent a transformation of preceding thought in the sense his 'external' social Darwinism does. This aspect is his attitude to evolutionary change. As noted above, Pearson was very concerned that the pressure of the working class against capitalism should lead only to gradual, controlled change. Indeed, he saw this as a key part of his role as an intellectual:

There are mighty forces at work likely to revolutionise social ideas and shake social stability. It is the duty of those, who have the leisure to investigate, to show how by gradual and continuous changes we can restrain these forces within safe channels, so that society shall emerge strong and efficient from the difficulties of our nineteenth-century Renaissance and Reformation.
(1888, 7)

In the Ethic of Freethought, from the introduction to which the above quotation is taken, one of his chief aims was to demonstrate/...

demonstrate the futility of attempting revolutionary rather than gradual change. The most striking piece of historical work in this volume was a description of the Anabaptist 'Kingdom of God in Münster' (1888, 263-314). He described in detail the organisational failings and eventual terrible fate of the millennial communists. In 'Socialism in Theory and Practice' he made his meaning clear to his working class audience. The examples of the Kingdom of God in Münster and the more recent Paris Commune demonstrated a law of history, that

... no great change ever occurs with a leap; no great social reconstruction, which will permanently benefit any class of the community, is ever brought about by a revolution. It is the result of a gradual growth, a progressive change, what we term an evolution. This is as much a law of history as of nature. (1888, 363; Pearson's emphasis)

As the last sentence indicates, Pearson sought to legitimate his gradualist socialism by an appeal to nature as well as history. 'Human progress, like Nature, never leaps; this is the most certain of all laws deduced from the study of human development' (1888, 122). Thus, in the 1880's Pearson formed a strong political commitment to a gradualist, non-saltatory view of evolution (see chapter six for some consequences of this).

This emphasis on gradualism was far from unique to Pearson. The very name of the Fabian Society was, of course, chosen to convey an insistence on gradualism. What is, however, of particular interest in Pearson's thought is the/...

the way in which gradualism is explicitly linked to a view of the role of 'head workers' in social change. Arguing for 'the enthusiasm of the study' as against 'the enthusiasm of the market-place', he wrote:

Human society cannot be changed in a year, scarcely in a hundred years; its organism is as complex as that of the most differentiated type of physical life; you can ruin that organism as you can destroy life, but remould it you cannot without the patient labour of generations, even of centuries. That labour itself must be directed by knowledge, knowledge of the laws which have dictated the rise and decay of human societies, and of those physical influences which manifest themselves in humanity as temperament, impulse and passion. (1888, 121)

Here it is the abstraction 'knowledge' that must be in command of social change. But in convincing his working class audience of the futility of revolution, it is the bearers of this knowledge who must command:

... the labourers with the hand will never be permanently successful in a revolution, unless they have the labourers with the head with them; they will want organisation, they will want discipline, and this must fail unless education stands by them. (1888, 363)

Of course, the Fabians would have agreed that it was the role of head-workers to organise and discipline the manual workers: thus the young Ramsay Macdonald called for socialism as a 'revolution directed from the study' (quoted, Hobsbawm, 1968, 258). Nevertheless, it is in Pearson's thought that we find the connections between gradualism and a leading role for head-workers especially clearly spelt out.

4.5 Pearson's Feminism

Pearson's Ethic of Freethought was regarded by his contemporaries as a daring book. The South African feminist Olive Schreiner wrote to Havelock Ellis:

You don't realise what a very brave thing a man in Pearson's position has done in printing that book at all. Anything approaching to that has never been published in England before by a professor in a college or university.
(27 January 1888. Quoted in Cronwright-Schreiner (ed.), 1924, 129.)

This was, however, not primarily because of the passages on socialism, rather because of those on the woman's question. The 'movement for the complete emancipation of women', together with the socialist movement, formed, in Pearson's opinion, the two most important movements of the time. Just as Pearson was, in the categories of his period, a socialist, he was also a feminist. At the same time, he had reservations about the women's movement that paralleled exactly those he had about the labour movement. As he wrote in an article of 1894:

It is almost idle to say what we wish women's future to be; the scientific attitude consists in endeavouring merely to trace the changes that are taking place, in sympathising with the difficulties and struggles of our fellow human beings under them, and finally, in trying so to direct, for we cannot possibly check, the revolutionary forces at work that they shall tend to the greater rather than the less stability of the body social.
(1897, 1, 243; Pearson's emphasis)

It was most important, he felt, that the women's movement should stop thinking and talking in terms of 'rights of women/...

women^t but should instead consider how women^t's role should best be reformulated to increase social efficiency and stability:

Not until the historical researches of Bachofen, Girard Teulon, and McLennan, with the anthropological studies of Tylor and Ploss, have been supplemented by careful investigation of the sanitary and social effects of past stages of sex-development, not until we have ample statistics of the medico-social results of the various regular and morbid forms of sex-relationship, will it be possible to lay the foundations of a real science of sexualogy. Without such a science we cannot safely determine whither the emancipation of women is leading us, or what is the true answer which must be given to the woman's question ... We have first to settle what is the physical capacity of woman, what would be the effect of her emancipation on her function of race-reproduction, before we can talk about her 'rights', which are, after all, only a vague description of what may be the fittest position for her, the sphere of her maximum usefulness in the developed society of the future. (1888, 371)

Pearson's feminism was real enough. He advocated the economic independence of women from men. He was prepared to take seriously, if not to endorse unequivocally, proposals for 'free unions' to replace conventional marriage (1888, 442-3). He argued that women had a 'duty to labour', and did not exclude from this professional work. Pearson accepted that change in gender roles was unavoidable and sought, as the above quotations indicate, to make sure that the women's movement was directed into channels that would not ultimately threaten the stability of his social world. Indeed, he saw how advantage could be taken of women joining the workforce. The Biometric and Eugenic Laboratories depended very heavily on women researchers, who could of course/...

course be paid less than their male counterparts.⁽¹⁹⁾

Although the employment of women in science was at this time still a rarity, Pearson was quick to see the gains that could flow from it:

The enormous number of women of the middle classes doing nothing, or busy over trivialities, is terrible to think of, when one sees in one branch of work only - scientific research - how much might be done by organised workers of every grade of capacity.
(1901d, 420 fn.)

Pearson was an early proponent of schemes for the 'endowment of Motherhood' (1888, 444). These schemes were of course adopted by the Fabians, and it may be that Pearson was the first person to convince Sidney Webb of their desirability (Webb to Pearson, 29 May 1887; Pearson Papers, CI D3).

Indeed, in the light of the overall similarities of Pearson's position on feminism and his position on socialism, it would seem not inaccurate to refer to his feminism as a 'Fabian feminism'.

Pearson's feminism affected his academic work in several ways. Much of his early historical work was devoted to developing the theory that pre-Christian Germany was a matriarchal society (1888, 395-426; 1897, 2, 1-245). It is possible that some of his work attacking traditional craniometry, and in particular the common assumption that brain size was an indicator of intellectual capacity, was motivated/...

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(19) See the financial records of these laboratories in the Pearson Papers, 244-5.

motivated by a desire to undermine the sexist use of craniometric arguments (Fee, n.d.). It is clear that Pearson's introduction of the coefficient of variation (standard deviation divided by mean) as a measure of variability resulted from his wish to deny the validity of the commonly asserted proposition that men are more variable than women. For most physical characteristics, such as height, when variability is measured by the standard deviation, men are more variable than women. When, however, the greater mean values of these characteristics for men are taken into account, through the use of the coefficient of variation, this is no longer true (Pearson, 1897, 1, 256-377). Nevertheless, by far the most important effect of Pearson's contact with feminism on his academic work was indirect. Feminism led him to serious consideration of heredity.

4.6 Pearson's Eugenics

Pearson's early writings (up to 1885) show no evidence of his having thought in any detail about heredity. At times, he inclined to a position that could be seen as hereditarian: for example, his remarks on the hierarchy of 'power intellectual' in Pearson (1881b).⁽²⁰⁾ At other times/...

- - -

(20) But even here he writes that 'power intellectual cannot be collected and bequeathed' (1881b, 270).

times, he appears environmentalist, as in his emphasis on the beneficial effects of education for the working class.

However, in 1885 he became a founder member of the 'Men's and Women's Club', a small circle of radical intellectuals devoted to 'the free and unreserved discussion of all matters in any way connected with the mutual position and relation of men and women' (quoted by E.S. Pearson, 1936-38, part 1, 210). It was in papers to this club that his early position on heredity was developed: a paper of 1885 on 'The Woman's Question', and one of 1886 on 'Socialism and Sex' (1888, 370-394 and 427-46).

The woman's question, Pearson noted, 'opens up great racial problems' (1888, 393). Among these were the problems that flowed from what he called 'the law of inherited characters' (1888, 390): that is, the tendency to the inheritance of mental and physical characteristics. 'The progress of the great mass of the people', he noted, 'is so dishearteningly slow'. The 'middle classes' have developed greatly in intellectual powers, 'but place a modern working man beside a mediaeval craftsman, and morally or intellectually should we be able to mark an absolute progress?' (1888, 390). It was suspected, he said, that the more highly educated men and women were having fewer children than the less educated. Must the state, or 'a strong public opinion', not intervene?

Shall those who are diseased, shall those who are nighest to the brute, have the right to reproduce their like? Shall the reckless, the idle/...

idle, be they poor or wealthy, those who follow mere instinct without reason, be the parents of future generations? Shall the consumptive father not be socially branded when he hands down misery to his offspring, and inefficient citizens to the state? It is difficult to conceive any greater race crime. Out of the law of inherited characters spring problems which strike deeply into the very roots of our present social habits.
(1888, 391)

Considerations such as these were central to Pearson in the reforms he wished to see in the social organisation of reproduction. He did indeed wish to see women freed from the absolute control of their husbands, but they should not thereby achieve full control over their reproduction. Part of the 'socialistic solution' of the sex problem was

... state interference if necessary in the matter of child-bearing, in order to preserve intersexual independence on the one hand, and the limit of efficient population on the other.
(1888, 445)

So, by the mid-1880's, Pearson was seriously entertaining the idea of the necessity of state interference 'in the family of the anti-social propagators of unnecessary human beings' (1888, 433). His eugenics emerged from his social and political thought, before he started academic work on heredity. It is therefore important to understand his eugenics as a social and political programme that was only later articulated scientifically. Pearson's Fabianism, his social Darwinism and his feminism fed into his eugenics, and he used eugenics as a 'scientific' mode of expressing his ideas in these fields. The transition in his political thought - to the extent that there is one - represented/...

represented the condensing of his ideas in these different fields into a single system best describable as eugenics. No radical break was involved. His adoption of eugenics did not, for example, entail his abandonment of socialism. Quite the opposite: he argued that a eugenic policy could not be successful under capitalism, chiefly because capitalist employers, desiring large supplies of cheap, unskilled labour, had an interest in maintaining the rate of reproduction of the 'unfit' at home and permitting large scale immigration of the 'unfit' from abroad (1888, 334-40). Socialism was thus a precondition for successful eugenics:

The pious wish of Darwin that the superior and not the inferior members of the group should be the parents of the future, is far more likely to be realised in a socialistic than in an individualistic state.
(Pearson, 1897, 1, 138)

Practical eugenics, Pearson wrote, is concerned with two fundamental problems:

- (i) The production of a sufficient supply of leaders of ability and energy for the community, and
 - (ii) The provision of intelligent and healthy men and women for the great army of workers.
- (1909e, 22)

In previous generations this had been achieved by natural selection. However, during the latter part of the nineteenth century, profound social changes had taken place, which had both reduced the impact of natural selection and brought into play countervailing forces.

Where the battle is to the capable and the thrifty, where the dull and idle have no chance to propagate their kind, there the nation will progress, even if the land be sterile, the environment/...

environment unfriendly and educational facilities small. Give educational facilities to all, limit the hours of labour to eight-a-day - providing leisure to watch two football matches a week - give a minimum wage with free medical advice, and yet you will find that the unemployables, the degenerates and the physical and mental weaklings increase rather than decrease. (1909d, 20-21)

The 'full purifying force of natural selection' had been suspended (Pearson, 1909c, 12) by humanitarian social and economic measures. The abolition of child labour, had, for the 'better class' of workers, turned a child from an economic asset to a straightforward expense (Pearson, 1909e, 7-9). Within the 'cultured classes',

... the child has never been an economic asset; it is a luxury which we know we must pay for, and expect to pay for, until after college and professional training, and, in the case of unmarried daughters, often long after our own lives are concluded. (Pearson, 1909e, 21)

While the 'better class' of workers and the 'cultured classes' had, in response to these pressures, restricted their fertility, the 'unemployables and degenerates', who could turn to public and private charity to support their offspring, had not done so. The resulting patterns of differential fertility, as revealed by Pearson's collaborator Heron (1906), were, Pearson argued, disastrous.

Pearson did not, however, desire a return to laissez-faire, nor think one practical.

Do I therefore call for less human sympathy, for more limited charity, and for sterner treatment of the weak? Not for a moment; we cannot go backwards a single step in the evolution of human feelings! (1909c, 25)

The/...

The struggle of national group against national group necessitated the growth of intra-group solidarity and of the ethical feelings associated with it. So the clock could not, as more right-wing eugenisists might suggest, be simply set back. The emotions associated with group solidarity must instead be channelled into rational forms, and artificial selection must replace natural selection.

... I demand that all sympathy and charity shall be organised and guided into paths where they will promote racial efficiency, and not lead us straight towards national shipwreck.
(1909c, 25)

The nation that first succeeded in doing this would be 'destined to be the predominant state of the future' (1911, 27).

Pearson as a eugenisist was thus no opponent of social reform as such. His argument was rather that reform should be 'scientific', and eugenics was to be the science that was to determine its scientificity. Two social premises, which can be traced from his political thought of the 1880's, were built into his eugenics. The first was the notion of a wide divide between 'working' and 'cultured' classes, with the latter the result of long selection for the mental abilities of the leader, co-ordinator and initiator. The second was of a working class divided into a 'better class' of skilled, hardworking and socially necessary workers, and a sub-proletariat of 'unemployables and degenerates', who, if not carefully controlled, would 'pollute/...

'pollute' the others. This last point is explicit in Pearson (1909c, 39) where he talks of 'that infernal lake which sends its unregarded rivulets to befoul more fertile social tracts'. Furthermore, Pearson's eugenics, like his feminism, can be described as 'Fabian'. He supported schemes for social reform such as national insurance and child allowances, which, he felt, would, if properly administered with regard for eugenic principles, increase the birth-rate of the 'better class' of workers relative to that of the sub-proletariat (1909e). He saw eugenics as a cohesive ideology, a secular religion which would enable the working class 'to see beyond the horizon of class-interest, [would] enable them to look upon the nation as an ever-changing organisation susceptible of advance or decay, as it obeys or disobeys stern natural laws' (1910b, 30-31). The 'Fabianism' of his eugenics emerges, however, most clearly in his conception of the nature of eugenics and of the best strategy for eugenicists. Referring to Bernard Shaw's eugenic 'extremism', Pearson wrote:

We may remind the Editor of 'Fabian Essays' that the doctrines of Eugenics will best be served, like those of socialism, by a slow process of impenetration.
(1914-30, 3A, 261)

While not eschewing support for particular eugenic measures such as the Mental Deficiency Bill (1912b, 28), Pearson's immediate goal was the establishment and institutionalisation of an academic science of eugenics, with

... its endowments, its special laboratory, its technical library and its proper share in the curriculum of academic studies.
(1909c, 3)

This/...

This academic discipline was then to be the basis of a campaign of persuasion directed at professionals, rather than the population at large, or politicians:

To produce a nation healthy alike in mind and body must become a fixed idea - one of almost religious intensity, as Francis Galton has expressed it - in the minds of the intellectual oligarchy, which after all sways the masses and their political leaders.
(1909c, 25)

Pearson was suspicious of the Eugenics Education Society, fearing the influence within it of amateurs and cranks. This was probably unjustified, given the high representation of the 'intellectual oligarchy' in the Society, but Pearson was concerned to the point of obsession with keeping the eugenics movement under the control of properly trained experts (see, for example, Pearson, 1914-30, 3A, 407).

If this control by experts was to be maintained, the facilities for training them, and the number of positions of power and influence open to them, must be greatly increased. Pearson was particularly interested in the scope for transforming the newly established profession of public health officer, by introducing training in both mathematics and medicine. He hoped in this way to develop a potential cadre of expert eugenists (Pearson, 1912b, 3-11). His own Biometric and Eugenic Laboratories were of course uniquely suited establishments for such training. For Pearson, as for the Fabians generally, one of the attractions of scientific social reform, whether 'socialist' or 'eugenist' in nature, was surely the growth of the occupational opportunities/...

opportunities for, and the status of, suitably trained experts and the teachers of these experts.

The controversies over tuberculosis and alcoholism throw interesting light on the differences in attitude between Pearson and the majority of eugenists. The latter controversy, especially, brought Pearson into direct conflict with leading members of the Eugenics Education Society. In these controversies, men who were often generally sympathetic to eugenics, but wished to preserve special status for a particular area of environmental reform (environmental health measures against tuberculosis, the temperance movement, etc.) came into conflict with the consistent hereditarianism of Pearson and his followers. In the case of tuberculosis, Pearson called for the restriction of the fertility of the tuberculous, claiming that this would, by eliminating inherited constitutional susceptibility to the disease, eliminate it, while public health measures would be inadequate. Pearson felt that cynical motives accounted for the beliefs of his opposition:

... quite recently and solemnly assembled in conclave, the wise men of medicine agreed that the constitution was an important factor in tuberculosis, but that it was not desirable to lay stress on it at the present time, for it would check the flow of public money into the fight against the tubercle bacillus. But what if the tubercle bacillus is actually committing suicide, or what if immunity be surviving without the aid of the expenditure of thousands of pounds of public money? Well, to say that, means that you will cut off the present or prospective occupation of a certain number of gentlemen who are fighting in one special manner the tubercle bacillus, and therefore, even if true, it must not be rashly said in public. (1911,12; Pearson's emphasis)

Without/...

Without necessarily accepting Pearson's account, it can be seen how particular occupational commitments could crosscut a general position. Pearson's occupational commitments were of a different nature to those of most eugenicists. His aim was the development of an academic science of eugenics, and thus he could afford this kind of consistency in the face of pragmatic counter-arguments.

A similar pattern was manifest in the controversy over alcoholism. In many ways, the eugenics and temperance campaigns were similar. Both sought social 'purification', and both attempted to improve the physical fitness of the working class, and to increase its amenability to social control, by the removal of a source of 'pollution'. Pearson and his co-workers suggested, however, that the causal chain in much temperance thought and propaganda should be reversed. Parental alcoholism was, they felt, merely the symptom of a general hereditary degeneracy and not, as temperance reformers suggested, the direct cause of feeble-mindedness and degeneracy in offspring. In this, Pearson was merely being a consistent hereditarian, but he infuriated fellow eugenicists who were supporters of the temperance movement. They saw a main plank of anti-alcohol propaganda ('it damages the children') being eroded.⁽²¹⁾ In this instance, Pearson/...

(21) This controversy is discussed in detail in Farrall (1970, 250-82). Note, once again, that Pearson's opponents are not being accused of logical inconsistency. Thus, it was perfectly logical to hold to a generalised hereditarianism but to argue that particular environmental 'race poisons' such as alcohol might have a direct chemical effect on the germ plasm. See, for example, Herbert (1910, 115).

Pearson was extending, into a new area, an idea already familiar in eugenic thinking, in a fashion unconstrained by pre-existing commitments in that area. His opponents, with their prior involvement in temperance reform, rejected this direct extension of eugenics, and instead sought ways of reconciling the rhetorical needs of temperance agitation and of eugenics.

4.7 Pearson's Philosophy

In the years up to 1892, Pearson developed a characteristic system of moral philosophy and of epistemology. The latter, as Norton (1978) has shown, had an influence on his eugenics and his Darwinism: he wished to develop these along 'proper' scientific lines, and 'proper' lines for him were positivist lines. In this he inevitably failed. It is easy to show that his actual scientific practice fell short of his own positivist criteria (see chapter seven). His philosophy was thus not a determinant of his science in any absolute sense. Nonetheless, his attempt to develop his science in such a way that it could be accounted good, positivist science did have an effect on its style and content, even if his practice escaped the bounds he sought to place on it. Similarly, while his moral philosophy perhaps did not have any independent influence on his science, it did form an important and revealing part of his system of thought.

Pearson's/...

Pearson's moral philosophy can be summed up in two maxims:

... morality is what is social, and immorality what is anti-social...

The ignorant cannot be moral.

(1888, 117, 122)

Pearson rejected all systems of absolute morality, such as Christianity, and he had no time for the ethical theories of those he saw as attempting to reintroduce such systems, such as Kant and the neo-Hegelians (1888, 117, 325). He put forward, instead, not an ethical relativism, as might at first be assumed from the statement 'morality is what is social', but an ethical naturalism. The basis of morality was the need for social coherence in the inter-group struggle for existence. Morality was not, however, simply the following of group norms. The truly moral actor had to take into account, not only the existing state of society, but also the direction of its evolution:

One thing only is fixed, the direction and rate of change of human society at a particular epoch. It may be difficult to measure, but it is none the less real and definite. The moral or good action is that which tends in the direction of growth of a particular society in a particular land at a particular time.
(1888, 428)

This is why 'the ignorant cannot be moral'. Only the individual who has knowledge of science and history, and who is therefore acquainted with the scientific laws of social evolution, can know which course of action is moral.

Pearson's moral philosophy, while in itself a not particularly/...

particularly unusual form of Victorian evolutionary naturalism, is interestingly related to other facets of his thought. As a radical and freethinker, he sought a foundation for morality different from the received morality of Christianity. As an intellectual and a professional, a view of morality that placed such a high premium on scientific knowledge had clear attraction for him. As a middle class socialist and a male feminist, he sought a weapon to use against the demands of more extreme socialists and feminists. Against revolutionaries in either field, he argued that to talk of the 'rights' of labour or of women was nonsense. Talk of 'rights' led too easily to revolutionary upheaval, he felt: it was 'the enthusiasm of the market place'. Consideration, instead, of the laws of social development, of the necessarily gradual nature of evolutionary change, led to moderation and the avoidance of revolutionary agitation, to 'the enthusiasm of the study' (1888, 115-34, 370-2).

If scientific knowledge was to play the role of the determinant of the nature of moral action, Pearson clearly needed to demarcate the boundary between properly scientific knowledge and mere belief. After all, many late Victorians claimed that science showed the futility and perniciousness of both socialism and the emancipation of women; some claimed that there were areas of knowledge over which science had no sway, and in which revealed religion had therefore to be relied upon; others claimed that/...

that new 'sciences' such as psychical research proved the existence of a spirit world. If Pearson was to maintain the social and political positions he held, if he was to be able to use 'science' as a tool in argument, and if he was to be able convincingly to legitimate 'scientific expertise', he needed a demarcation criterion that could be deployed against arguments such as the above. It was arguably for this reason that he was led to develop a philosophy of science.

This philosophy of science was presented in the Grammar of Science (1892a). It was founded on a positivist and phenomenalist epistemology. All knowledge, Pearson argued, was based on sense-impressions; it was impossible meaningfully to discuss unknown and unknowable 'things-in-themselves' that metaphysicians saw as lying behind sense-impressions. The task of science was simply to describe as economically as possible the 'routine of perceptions'. Concepts that were firmly based on experience, and those that contributed to economy of description, were allowable: others were to be banished. The sphere of science as thus delimited was co-extensive with the sphere of all valid knowledge. Certainly, there were types of phenomena that had yet to be satisfactorily described by science, but there were no phenomena to which the scientific method was not applicable. What was not science was simply not knowledge. Pearson's was an idealist position. For him, the referent of scientific law was the routine of human perception/...

perception, not the unknowable physical universe: 'matter' was as redundant and unscientific a concept as 'spirit'. On the other hand, his philosophy did not carry the anti-scientific, or at any rate anti-scientistic, connotations frequently associated with idealism. Indeed, it is interesting that a genuinely 'spiritualistic' idealist such as St. George Mivart should perceive Pearson's position as de facto materialist (Mivart, 1895).

Pearson's philosophy integrated well with the other elements of his position. It was an apt underpinning for a man seeking progressive reforms, and wishing moral sanction for these, while wanting to avoid an appeal to the morality of the market-place. Morality was based on evolutionary law. Thus its progressive nature was guaranteed, and only those who had knowledge of science and of social development could claim the label 'moral'. Pearson's positivism was a useful element in his struggle against anti-scientistic reaction and those he saw as allied within science. His epistemology, as developed in his philosophy of science, was a superb polemical weapon for revealing the 'superstition' and 'metaphysics' in the thought of his opponents.

Pearson was not the only thinker to deploy philosophy in this way. The philosophy of Mach, with its phenomenalism and ethical relativism, was taken up by scientific and political radicals (such as Lenin's 'ultra-left/...

left' opponent Bogdanov) as a weapon against 'establishment' science and as a possible scientific justification for radicalism through an operationalised Marxism and an ethical relativism (on this see Feuer, 1971).⁽²²⁾ While Mach's followers took their radicalism further than Pearson and his followers (the Machian, Friedrich Adler, chose to assassinate a Prime Minister), the general connotations of the two philosophical systems were similar. It is perhaps in this context that one should see the reason for the success of Pearson's philosophy of science in attracting young, radical scientists of the period up to 1914.⁽²³⁾

4.8 Pearson's Science

Up to 1892, Pearson's directly scientific work lay within a very orthodox tradition of applied mathematics. From that year on, it underwent a complete transformation. The purpose of this section is to present an explanation of this shift, and to attempt to show the relevance to an understanding of this shift of knowledge of Pearson's wider beliefs and aims. By the early 1890's, Pearson had developed the distinctive political and philosophical position analysed above, with its interlinked elements of Fabian socialism, reformist feminism, emerging hereditarianism, social/...

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(22) For the relations between Pearson and Mach, see Thiele (1969).

(23) For one testimonial to its success, see J.B.S. Haldane (1957, 430).

social Darwinism, ethical naturalism, and a positivist and phenomenalist epistemology. The conjuncture of this ideological position, the high level and particular character of his mathematical training and competence, and at least one important 'accident', resulted, it will be argued, in a move into work in statistical theory of a particular kind. (24)

Four phases in Pearson's involvement with his new line of work can be identified. The first was the period up to the beginning of 1891. The most important piece of evidence from this period is a paper, read by Pearson to the Men's and Women's Club, on Galton's Natural Inheritance (Galton, 1889b; Pearson, 1889). (25) It was indeed natural that Pearson, given his interest in heredity and his mathematical knowledge, should turn to Galton's work. Nor is it surprising that, given his pre-existing beliefs about heredity and his speculations about eugenics, he should find what he read exciting and important. He wrote:

The general conclusion one must be forced to by accepting Galton's theories is the imperative importance of humans doing for themselves what they do for cattle, if they wish to raise the mediocrity of their race.
(Pearson, 1889, 34)

He/...

(24) At least one other individual, Arthur Black, shared a broadly similar conjuncture of factors, except that the circumstances of his life were much less happy than that of Karl Pearson. Black will be discussed in chapter five.

(25) I am grateful to Professor E.S. Pearson for allowing me to see this paper.

He was, however, not immediately attracted to Galton's quantitative approach:

Personally I ought to say that there is, in my own opinion, considerable danger in applying the methods of exact science to problems in descriptive science, whether they be problems of heredity or of political economy; the grace and logical accuracy of the mathematical processes are apt to so fascinate the descriptive scientist that he seeks for sociological hypotheses which fit his mathematical reasoning and this without first ascertaining whether the basis of his hypotheses is as broad as that human life to which the theory is to be applied. I write therefore as a very partial sympathiser with Galton's methods.
(Pearson, 1889, 2)

Pearson's initial attraction to Galton's work thus appears to have been the result of its substantive, rather than methodological, importance. It was as an 'amateur' student of heredity and eugenics that Pearson first came into contact with Galton's statistical theory.

No immediate consequences followed from this first contact. At this stage in his career Pearson's energies were chiefly devoted to the preparation of The Grammar of Science, and to the Gresham Lectures on Geometry in which many of the ideas of the Grammar were first presented (E.S. Pearson, 1936-8, part 1, 212-6). The work on the Grammar involved some consideration of the concept of probability, and Pearson used Bayes's theorem to justify inductive inference (1892a, 166-79). This hardly broke new ground, but there were some indications in the Grammar of the developing direction of Pearson's thought. One of the four 'claims of science' discussed by Pearson was that science/...

science 'can on occasion adduce facts having far more direct bearing on social problems that any theory of the state propounded by the philosophers from the days of Plato to those of Hegel' (1892a, 31; Pearson's emphasis). The example Pearson gave of this was Weismann's theory of the non-inheritance of acquired characteristics:

Now this conclusion of Weismann's - if it be valid, and all we can say at présent is that the arguments in favour of it are remarkably strong - radically affects our judgment on the moral conduct of the individual, and on the duties of the state and society towards their degenerate members. No degenerate and feeble stock will ever be converted into healthy and sound stock by the accumulated effects of education, good laws, and sanitary surroundings. (1892a, 32)

In the Grammar, we also find Pearson's positivist criteria of valid knowledge turned against much biological thinking, in particular that of the opponents of Darwinism. The task of the biologist was simply to describe, as economically as possible, the phenomena, and not to attempt to explain them:

... the failure to grasp [this distinction] has been made the ground for what is really a meta-physical attack on the Darwinian theory of evolution. As I interpret that theory it is truly scientific, for the very reason that it does not attempt to explain anything. It takes the facts of life as we perceive them, and attempts to describe them in a brief formula involving such conceptions as 'variation', 'inheritance', 'natural selection', and 'sexual selection' ... Perhaps some of the modern critics of Darwin will be less ready to consider adaptations as 'not explicable' by natural selection, but due to the 'precise chemical nature of protoplasmic metabolism', or to 'an internal fate, expressible in terms of dominant chemical constitution', if they once grasp that physics and chemistry in their turn render nothing 'explicable', but merely, like natural selection itself, are shorthand descriptions of changes in our sense-impressions. (1892a, 421-2)

The/...

The second phase of Pearson's change of direction in his mathematical work, foreshadowed as it was in the Grammar, did not come until after its publication. At the beginning of 1891, W.F.R. Weldon became Professor of Zoology at University College, London. Weldon, who will be discussed in chapter five, had independently developed an interest in Galton's work, though from a professional biologist's rather than political thinker's point of view. He had already published work using Galton's statistical methods (Weldon, 1890), and had tried unsuccessfully to interest mathematicians at Cambridge in this area (Pearson, 1906, 283). Pearson and he almost immediately began an intense intellectual collaboration and personal friendship. It was this contact with Weldon that led to Pearson's first original work in statistical theory. Pearson's interest in the field was developing rapidly. He chose to deliver some of his Gresham Lectures on the 'Geometry of Statistics'. These began in orthodox enough fashion, with an account of the historical development of statistics, then of graphical methods of data presentation and of the laws of probability (E.S. Pearson, 1936-8, part 1, 214-6). But already the terminology Pearson used was altered to take account of the move away from the traditional area of error theory to other applications:

... in November 1892 variation is measured by the mean error; by November 1893 the standard deviation has been introduced and the curve of error becomes the normal curve.

(E.S. Pearson, 1936-8, part 1, 216; E.S. Pearson's emphasis)

In/...

In October 1893 Pearson sent to the Royal Society his first paper on statistical theory. Its title indicates the context in which it was written: 'Contributions to the Mathematical Theory of Evolution' (Pearson, 1894). It dealt with the dissection of frequency curves into normal components, and the applications were drawn from data supplied by Weldon. A second paper followed a year later, and discussed the fitting of skew frequency curves to observational data (Pearson, 1895). In this paper the famous Pearson system of frequency curves, all derived from one fundamental differential equation, was first presented.

Unquestionably, the 'accident' of Pearson's being brought into contact with Weldon was important in this phase of the transition in Pearson's work. The stimulus of conversations with Weldon and of the problems thrown up by Weldon's data were undeniably important in the production of these two initial papers. It does not, however, seem justifiable to attribute to this accident the extreme determining role suggested by J.B.S. Haldane (1957, 431), who concludes that if Pearson had met, instead of Weldon, an economist or engineer interested in the variation of manufactured goods, he would have started work in industrial quality control rather than biometric statistics. Weldon acted as the 'trigger' for intellectual developments the roots of which it is possible, as we have seen, to trace back in Pearson's thought well before the contact with his fellow/...

fellow professor.⁽²⁶⁾ It is not very useful to speculate on what might have happened if Weldon had not been appointed to University College: suffice it to say that the contact between Pearson and Weldon was only one contributory factor in the transition here examined.

Pearson's first two statistical papers might conceivably be interpreted as the work of a man who saw in biology a sphere for the application of his mathematical skills, but which had no intrinsic interest for him. His next statistical paper (Pearson, 1896) and the two volumes of essays, The Chances of Death (1897), demonstrate the falsity of this view. They can be taken as indicators of a third phase in this transition. Pearson himself wrote of this phase:

Now, if you are going to take Darwinism as your theory of life and apply it to human problems, you must not only believe it to be true, but you must set to, and demonstrate that it actually applies. That task I endeavoured to undertake after the late Lord Salisbury's famous attack on Darwinism at the Oxford meeting of the British Association in 1894. It was not a light task, but it gave for many years the raison d'être of my statistical work.
(1912b, 11; Pearson's emphasis)

Salisbury (1894) had suggested that the process of natural selection could not be demonstrated, but was merely an implausible hypothesis. He called for a return to the principle of creative design. While too much weight should not be placed on this incident alone, as again the development of/...

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(26) See also Norton (1978).

of Pearson's thought lay in this direction anyway, it should be noted that Salisbury was the epitome of what Pearson opposed in the upper classes of British society. The religiously motivated attack on Darwinism from the High Tory peer led to an immediate riposte from Pearson, first published in the Fortnightly Review of 1894 and reprinted in the Chances of Death (1897, 1, 140-172). Pearson attacked Salisbury as a representative of 'reaction' and the 'new bigotry' and claimed that 'the theory of evolution is likely to become a branch of the theory of chance', and that when this happens views like Salisbury's would obtain 'very poor comfort' as a 'quantitative measure of the rate of natural selection' was found (1897, 1, 172, 167).

Pearson's work of the 1890's on the quantification of the theory of evolution can thus be seen as an attempt to defend Darwinism against its opponents, with a view to its application to 'human problems'. Pearson's political position can be seen as operative here in two ways: firstly, in the potency for him of Darwinism as a cultural symbol representing the struggle of the scientific intellectual against 'reaction'; secondly, in the crucial importance to him of developing an intellectually 'hard' and applicable social Darwinism. It would be mistaken to see biometry (the name he gave to this new area of study) as simply the application of statistical techniques to biology. The fact that Pearson took up this study, and the way he developed it, were both conditioned by the political role of/...

of his evolutionary theory. Further, his positivism was also of importance in conditioning the form in which he developed evolutionary biology. He sought a theory of evolution and heredity that could be presented as simply a summary of the 'facts' of the biological world: a statistical approach was therefore clearly suitable (Norton, 1978).⁽²⁷⁾

Pearson's third 'Mathematical Contribution to the Theory of Evolution' (Pearson, 1896) best illustrates the nature of Pearsonian biometry. This was a very ambitious and important paper, in which Pearson put forward the now standard product-moment expression for the coefficient of correlation and developed a large part of the theory of multiple correlation and regression. These developments in statistical theory arose from the attempt in the paper to produce a quantitative theory of evolution on the basis of formal definitions of various concepts of the theory. The paper should not, however, be seen as a general, disinterested biological monograph. In a real sense it was about human beings in society. The definitions are/...

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(27) This should not be taken as implying either that his biometric theories in fact fulfilled strict positivist criteria, or that his philosophical views were the only reason he chose to develop a statistical evolutionary theory. On the latter point, it should be noted that Darwinism was already an implicitly statistical theory (although not expressed quantitatively); that Pearson's competences lay, not in biology, but in mathematics of a particular application-oriented type (notably continuum mechanics); and that Pearson had before him a potent exemplar in Galton's work on statistical biology.

are general: but it is clear that man was the paradigm organism to which they were intended to apply. To gain the type of data necessary for the application of these definitions to an animal population in the wild would be a daunting task for biologists even nowadays. Further, in writing this paper Pearson had a political purpose. He wished to refute the theory that, should natural selection be suspended and random mating take place, a species would revert to an original 'species type'. This theory, originally put forward by Weismann, and referred to by Pearson as the doctrine of 'panmixia', had been given wide circulation by Benjamin Kidd in his popular Social Evolution (1895; first published in 1894). Kidd wrote:

... if all the individuals of every generation in any species were allowed to equally propagate their kind, the average of each generation would continually tend to fall below the average of the generation which preceded it, and a process of slow but steady deterioration would ensue.
(1895, 37; emphasis deleted)

This law, Kidd concluded, proved the impossibility of the long-term success of a socialist society: with the struggle for survival suspended, degeneration would automatically follow. Pearson immediately attacked this anti-socialist doctrine in a paper in the Fortnightly Review of July 1894 (reprinted in Pearson, 1897, 1, 103-39) and backed up his point of view with a detailed quantitative analysis in the 1896 paper (Pearson, 1896, 298-318). His aim was to show that the efficacy of selection was greater and more permanent than the doctrine of 'panmixia' allowed. To do this, he had/...

had to modify Galton's interpretation of regression. Galton had assumed that regression took place to a fixed racial mean, and in this form the theory of regression indeed suggested that the suspension of natural selection could lead to the consequences suggested by Kidd. Pearson saw that to avoid these consequences it was necessary to assume 'that successive selections must connote some progression of the focus of regression' (1896, 307). On the assumption that 'the focus of regression is the mean of the population from which parents have been selected' (1896, 308), it was possible to show that

... with the correlation coefficients of inheritance anything like their value in man, five generations of selection of the type required in both parents would suffice to establish a breed.
(1896, 317; emphasis deleted)

The subsequent suspension of selection would not then be followed by any automatic deterioration.

Thus, we see how Pearson's quantitative analysis could have a political import. It was possible to use it to argue for the efficacy of a 'socialist' policy, in which, for the sake of group solidarity in inter-imperialist struggle, intra-group competition - and the resulting natural selection - was suspended and replaced by conscious eugenic selection. No deterioration would ensue: indeed careful eugenic selection would permanently improve the quality of the race. From the point of view of Darwinism and eugenics, socialism was superior to an individualism

... which/...

... which to-day seems directly to encourage the unlimited breeding of the physically and mentally most degenerate classes in the community, and refuses to impose any test as to physique or intellect on the pauper aliens it allows to enter the social group.
(1897, 1, 137-8)

Further, it is clear from Pearson (1896) that, in his biometric work, Pearson was by no means simply developing technical tools to aid others' theories. He was prepared, for reasons which lie in his attitudes to politics and Darwinism, to modify essential substantive components of Galton's theories, such as the evolutionary interpretation of regression. Finally, it is possible to understand, reading Pearson's reply to Kidd, the polemical force of the quantitative apparatus Pearson was able to deploy. To take just one example:

While at the sources of knowledge vague descriptive reasoning is being succeeded by a more just quantitative theory of evolution, the innumerable conduit pipes represented by popular writers and the press are still providing the public with a fluid so contaminated with the germs of muddle-headedness that it is little wonder if whole classes of the community are poisoned. I venture accordingly to make the following definite statement:- That until the quantitative importance and numerical relationship of the various factors, vaguely grouped together as the theory of evolution, are accurately ascertained, no valid argument can be based on the theory of evolution with regard to the growth of civilised human societies. We must remain agnostic as to these problems until the theory of evolution has been readjusted on its new basis.
(1897, 1, 105)

For Pearson, the 'readjustment' to the 'new basis' of the theory of evolution, was a project of great importance.

The theory of evolution

... is not merely a passive intellectual view of nature; it applies to man in his communities as it/...

it applies to all forms of life. It teaches us the art of living, of building up stable and dominant nations, and it is as important for statesmen and philanthropists in council as for the scientist in his laboratory or the naturalist in the field.

(Pearson, 1900a, 468)

It was Pearson's chosen task to purge this theory of metaphysics and to place it on a sure, incontestable, quantitative and positivist framework. In doing this, he was working on an enterprise that expressed all the major elements of his political and philosophical position.

By this third phase, Pearson's transition to work in biometric statistics was essentially complete. The fourth phase, from the end of the 1890's onwards, was one of consolidation and of the building of a research team. The problems dealt with by Pearson began to include a wide range of puzzles thrown up as a by-product of the main thrust of biometric research. A good example of this was his development of the chi square test. The work on curve fitting of the early 1890's led to a need for some measure of the goodness-of-fit of a theoretical curve to observed data. Weldon's attempts to develop (through dice tossing) empirical stochastic models for frequency distributions led to a similar need. E.S. Pearson (1965, 330-337) documents the early efforts by Weldon and Pearson to solve these problems. W.F. Sheppard, who will be discussed in the following chapter, attempted to solve the problem, but was defeated by his failure to cope with the correlation between the divergences in different cells of a frequency distribution (see/...

(see Lancaster, 1969, 12). Pearson - who was able to draw on his knowledge of multivariate correlation, a knowledge gained in his studies of heredity and evolution - finally developed a solution to the problem (Pearson, 1900c). We can see here a problem arising in a derivative fashion from biometric statistical work, and being solved in a way not directly conditioned by this work, except in that concepts directly developed in this work provided a resource for its solution. Nevertheless, even in this phase the general nature of Pearson's programme of scientific research remained conditioned by the overall social and political aims of its founder.⁽²⁷⁾ Indeed the specific part played by eugenics, rather than social Darwinism generally, was, as will be shown in chapter seven, to increase.

4.9 Conclusion

The relationship between the starting-point of the analysis of this chapter - the structural situation of the professional middle class - and its end - the details of Pearson's science - involves a long chain of mediations. The two crucial mediations are first, the claimed role of Pearson as an 'exceptional individual' expressing a professional middle class ideology in particularly clear and consistent form; and second, Pearson's social, political and philosophical beliefs as conditioning his science. This/...

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(27) See chapters six and seven.

This second point has, I hope, been demonstrated. The first point, is, however, inevitably more tentative, as it involves a move far beyond the kind of documentary evidence that can be used to justify the second. It is, therefore, perhaps apt to conclude this chapter with a review of the 'exceptional individual' argument as applied to Pearson. (28)

Pearson's overall intellectual position was unique. Particular aspects of it - Fabianism, social Darwinism, reformist feminism, eugenics, ethical naturalism, positivism - were of course shared in varying degrees by late Victorian professionals. But with the possible exception of a handful of Pearson's followers, the overall 'mix' of elements is not, to my knowledge, to be found exactly replicated in any other individual. The crucial point made here - which distinguishes the approach to the sociology of knowledge associated with Lukács and Goldmann from any empiricist, statistical approach - is that this uniqueness in no way invalidates the analysis of Pearson's system of belief as one appropriate to the professional middle class of late Victorian Britain. This analysis is, of course, theoretical in its nature. It cannot be demonstrated that Pearson consciously and deliberately set out/...

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- (28) The sociological claim relating particular sets of ideas to particular social interests (in this case those of the professional middle class) could, of course, stand independently of the 'exceptional individual' hypothesis. The validity of the latter supports the former, but the invalidity of the latter does not necessarily imply the invalidity of the former.

out to create a professional middle class ideology. Although some of his early writings clearly show him in search of an appropriate belief system, the role of class interests in shaping this can only be presumed, and it is certainly not to be expected that he would necessarily be conscious of them. If the analysis of Pearson's beliefs and of the structural position and interests of the professional middle class presented here is accepted, then we have an instance of the 'match' of ideas and social position. Explaining why this 'match' came about exactly when it did, and why the particular individual Karl Pearson should have manifested it, is, however, beyond the present capacity of the sociology of knowledge. In the last analysis, it is not necessarily a sociological problem.

This does not mean, however, that all we can do is to point to this one instance of a 'match'. It is possible to look at the relationship between the historical fate of a system of belief and that of the class to which it is imputed. Ideologies are of course context-bound, and there is no reason to expect a permanent attachment of particular ideas to particular classes in changing cultural and historical circumstances. Nevertheless, at least some regularities can surely be expected. Take Fabianism, for example. The growth of the professional middle class (and of state bureaucracy and social intervention) that has taken place since 1914 has meant that Fabianism has changed from a minority belief to a dominant ideology. It is no longer radical/...

radical to talk of experts, scientific administration and politics, or selection on merit, nor to demand an expansion in the role of the state. Similarly with eugenics. While negative eugenics as a programme of social control proved context-bound, many of the eugenicists' psychological ideas became widely accepted. The relatively recent reaction against them within sectors of the professional middle class, itself an interesting problem for the sociology of knowledge, should not blind us to the ideological success of the hereditarian theory of mental ability. A reaction has also set in against scientific positivism of the Pearsonian kind, but the ideas set out in the Grammar of Science, even if they would be rejected by professional philosophers, would not be wholly unacceptable to many contemporary scientists.⁽²⁹⁾ The employment of women as researchers and in other professional and 'para-professional' positions has become commonplace. The particular form of Pearson's reaction against individualistic social Darwinism is outdated, but the notions of collectivism, and of the development of internal cohesion against external threat, have enjoyed considerable twentieth century success.

It would, therefore, not be correct to dismiss Pearson's ideas as simply those of an idiosyncratic individual/...

(29) Not, that is, in their technical detail - Pearson's phenomenalism was, even its time, alien to the experience of working scientists, who had difficulty in treating atoms, genes, etc., as anything other than real entities - but in terms of the general image of science presented.

individual. It is too easy to focus on aspects that were discarded and now seem outlandish, and to forget those that became the common-place beliefs of the professional middle class of at least the recent past, if not the present. On the whole, the ideas embraced by Pearson were ideas growing, rather than declining, in their historical importance. This growth can surely be attributed to the growth of the professional middle class and its social role: Fabianism, the 'IQ cult', positivism, and so on, grew as professional administrators, teachers and psychologists, social and natural scientists became more important. On the other hand, Pearson as an individual, while at least moderately famous as a general intellectual in the Edwardian period, never enjoyed the cult status amongst the professional middle class that, say, Herbert Spencer enjoyed amongst the American - and to a lesser extent, British - bourgeoisie and petty bourgeoisie. In full accord with his own views on the social responsibility of the scientific intellectual, Pearson eschewed opportunism. He never made the compromises that would have been necessary to become leader of a social movement such as Fabianism or eugenics.⁽³⁰⁾ That does not mean, however, that the ideas he put forward should be seen as unsuccessful ideas.

The analysis of Pearson presented here does differ in/...

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(30) It is interesting to compare the relations between Pearson's thought and that of the majority of Fabians and eugenicists, with the relations between Pascal's thought and that of the 'moderate' Jansenists, as described by Goldmann (1964).

in its nature from that by Goldmann (1964) of Pascal and Racine, in which Goldmann's sociology of knowledge is best developed. Goldmann's argument rests, ultimately, on a claimed structural homology between Jansenism, as expressed by Pascal and Racine, and the social situation of the class, the noblesse de robe, to which Jansenism is imputed. The analysis of Pearson does not depend on structural parallels, but rather on imputations of class interests. Further, Goldmann makes much of the aesthetic coherence of the ideas of his principal subjects. The coherence found in Pearson's work is not of this nature: it refers instead to what I claim to be the relative freedom of Pearson's thought from the 'noise' generated by particularistic interests. These two major reservations aside, it is to be hoped that this chapter has shown that the type of analysis pioneered by Goldmann can be of use in understanding aspects of the relationship between individual thinkers and their class positions. A sociological approach need not be restricted to relatively large-scale movements such as eugenics, but can also be used to analyse the work of unique individuals such as Karl Pearson.

Chapter Five

The Emergence of Statistical Theory as a Scientific Specialty

Up to this point, discussion of statistical theory has been focused almost exclusively on only two men, Francis Galton and Karl Pearson. Certainly, they were central to the development of British statistics, but, in order to give a rounded picture of the context in which they worked, it is necessary to consider other, less prominent, individuals, and also institutional developments. It has been convincingly argued (notably by Ben-David and Collins, 1966) that good and productive ideas alone are not sufficient for the foundation of a new scientific specialty. Several things must happen before we can talk of the emergence of a new specialty: a network of scientists interested in the new field must develop; means of communication between them, both formal and informal, must be established; a mechanism must be devised for recruitment to, and training in, the field, and this mechanism must be given some stable form; sufficient financial and other resources must be obtained to permit the foregoing.

A considerable literature has developed over the last few years which employs this perspective in the discussion/...

cussion of the growth of scientific specialties,⁽¹⁾ and useful provisional summaries of it are given by Edge and Mulkay (1975) and in the introduction to Lemaine et al (eds) (1976). Perhaps the most useful way to conceptualise the problem is to see the development of a new discipline as analogous to the development of a new political party. Seen in this light, some of the points raised in this literature become strikingly familiar. Compare, for example, Griffith and Mullins (1972) with Lenin (1947): both works emphasise the crucial role of tightly-knit and coherent groups, even if small, in promoting revolutionary change.

Consider the above list of necessary conditions for the development and institutionalisation of a new discipline. Do they not apply also to the development of a new political party? It too must be based on a network of committed individuals, who must develop means of communication with each other. It too needs to recruit, and to develop the ideological and other competences of those it recruits. Notoriously, it too requires material resources. However, the analogy of the political party suggests/...

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(1) For example, Ben-David (1960), Ben-David and Collins (1965), Hagstrom (1965, 159-253), C.S. Fisher (1967), T.N. Clark (1972; 1973), Cole (1972), Oberschall (1972), Mulkay (1972; 1974; 1975; 1976a; 1976b), Mulkay and Edge (1973), Edge and Mulkay (1975; 1976), Mulkay, Gilbert and Woolgar (1975), Mullins (1972; 1973a; 1973b; 1975), Griffith and Mullins (1972), Law (1973), Law and Barnes (1976), Whitley (1974; 1975), Stehr (1975), Krohn and Schäfer (1976), Dolby (1976), Worboys (1976), Van der Daele and Weingart (1976), Chubin (1976), Johnston and Robbins (1977).

suggests that it may well be misguided to search for a single set of factors governing the development of scientific specialties. Political environments differ. Factors which promote the successful growth of a party in one environment (for example, a highly centralised internal structure) may hinder its growth in another. Funds may be most readily available from one source (business concerns, say) in one situation, and from another (mass subscriptions) in another. Similarly with scientific disciplines. Access to graduate students, say, may be necessary for growth under the normal conditions of the scientific enterprise in industrialised societies, but it can hardly be a universal factor. The creation of a new journal may sometimes be necessary, sometimes not.

So the perspective taken here will not be an attempt to list a set of factors that are present or absent in the development of British statistics. Rather, Galton and Pearson will be considered as the nucleus of a scientific 'party', attempting to build networks, to establish adequate means of communication, recruit and train others and to gain resources to do so. An attempt will be made to understand the situation in which they operated, and how particular institutional structures and ideological contexts helped and hindered them in their enterprise. Further, lest this framework be thought too voluntaristic, attention will be paid to the 'side bets' (Becker, 1960) involved in the process. Interests extraneous to the original/...

original enterprise, became involved in it, and transformed its nature, independently of the conscious intention of those who initiated it. The process of the formation of 'side bets' is, of course, familiar to students of politics. In attempting to promote change, reformers frequently develop a stake in the very institutions they have set out to alter or destroy. Nothing as complete as this happens in the case of British statistics, but it is clear that the enterprise that developed largely as a result of the efforts of Galton and Pearson was not shaped by their initial intentions alone.

5.1 Galton and the Mathematicians

In 1892 Francis Galton wrote to the former Senior Wrangler, W.F. Sheppard:

What is greatly wanted is a clean elegant résumé of all the theoretical work concerned in the social and biographical problems to which the exponential law has been applied. I believe the time is ripe for any competent mathematician to do this with much credit to himself. I am not competent and know it... I have often considered what seems wanted and been very desirous of discovering someone who was disposed to throw himself into so useful and such high-class work. He might practically found a science, the material for which is now too chaotic.

(Quoted in K. Pearson, 1914-30, 3B, 486-7;
Galton's emphasis)

Galton had tried long and hard to generate consistent interest by a 'competent mathematician' in the mathematical and statistical aspects of the problems on which he was working. Up to 1892, he had sought to develop active collaboration with/...

with at least seven such men: H.W. Watson, Donald MacAlister, J.D. Hamilton Dickson, John Venn, S.H. Burbury, W.F. Sheppard, and Francis Ysidro Edgeworth. While none of these collaborations was sterile, none produced the fruitful results of Galton's contact with Karl Pearson. It is, therefore, worth contrasting these former with the latter, and also to discuss one further mathematician, Arthur Black, who, although he never met or corresponded with Galton, might have contributed more than any of the others apart from Pearson, had his career not been terminated by his suicide in 1893.

Galton's relationships with the first six are discussed in appendix C. All six were Cambridge graduates, and all were highly placed in the Tripos examination in mathematics. Only Hamilton Dickson pursued a career exclusively in university teaching and research in mathematics. The others spent at least part of their lives in the established professions: Watson and Venn, the church; MacAlister, medicine; Burbury and Sheppard, the law. One might indeed suspect that men like this, academically trained but marginal to any established career structure in mathematics, might be ideally suited to the role of innovator in an applied mathematical field. This may have been the case, but, with the partial exception of Sheppard, they all seem to have lacked commitment to Galton's particular project. The pattern in each case is similar. Each became interested in a particular problem or aspect of Galton's work, investigated/...

investigated it mathematically, and having done so dropped it and returned to his own pursuits. In modern parlance, their role was almost that of the 'consultant', except that it was the chance to display their mathematical competences on an interesting problem - and perhaps the flattering contact with a highly prestigious man like Galton - that motivated them, rather than financial reward.

Galton's contact with W.F. Sheppard, although it was productive of a much larger body of work than his contact with the other five, was not qualitatively dissimilar. Sheppard's initial interest in Galton's work may have been sparked by its eugenic applications, but his prime motive seems to have been simply that, in first Galton's and then Pearson's work, he found an excellent area for the application of his particular skills. Where Galton and Pearson had provided the key concepts, he followed with detailed investigation and tabulation. He was particularly competent in what would now be called numerical analysis. He drew up the first modern tables of the normal curve using the standard deviation as the argument (Sheppard, 1903). He proved a moderately important theorem in bivariate normal correlation, which is now sometimes known as Sheppard's theorem on median dichotomy (Sheppard, 1898b). In chapter eight his work on the calculation of probable errors will be discussed. In general, though, it can be said that it is not unfair to the man, with his great concern for precision and numerical accuracy, that his name should have gone/...

gone down in the history of statistics primarily as the inventor of a correction formula: Sheppard's formula for the correction of moments estimated from grouped data, first presented in Sheppard (1897b).

Unlike these six mathematicians, Francis Ysidro Edgeworth (1845-1926) began work on statistical theory independently of Galton.⁽²⁾ Apparently self-taught in mathematics, he was educated in modern languages at Trinity College, Dublin and in classics at Oxford, and seems to have then spent some years practising law. In 1880 he became Lecturer in Logic, and in 1888 Professor of Political Economy, at King's College, London. In 1891 he was appointed Drummond Professor of Political Economy at Oxford.

Utilitarianism formed the basis of his early work, notably Edgeworth (1877; 1881). His utilitarianism was, however, not at all radical in its thrust. Thus Edgeworth claimed that when

... we calculate the utility of pre-utilitarian institutions, we are impressed with a view of Nature, not, as in the picture left by Mill, all bad, but a first approximation to the best. We are biassed to a more conservative caution in reform. And we may have here not only a direction, but a motive, to our end. For, as Nature is judged more good, so more potent than the great utilitarian has allowed are the motives to morality which/...

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- (2) For biographical studies of Edgeworth see Keynes (1926), Bowley (1934) and Kendall (1968); the only comprehensive bibliography of Edgeworth's extensive writings is H.G. Johnson (n.d.). A.L. Bowley (1972; first published in 1928) is a valuable guide to Edgeworth's frequently obscure work in mathematical statistics. His work on inference is discussed in Pratt (1976).

which religion finds in the attributes of God.
(1881, 82)

Edgeworth appears to have first made use of Galton's work in order to justify the removal of any egalitarian implications from the utilitarian goal of maximising happiness. Edgeworth argued that individuals differed in their capacity for happiness and that to maximise total happiness more of the 'means of happiness' should be given to those most able to enjoy them.⁽³⁾ He then had to answer the objection that the capacity for happiness might be the result of education. Edgeworth argued that this would be incompatible with 'what is known about heredity' (1881, 59). Citing Quetelet and Galton, he argued that the distribution of capacity for happiness was normal, and that the offspring of parents with a given capacity for happiness would have capacities for happiness distributed normally round those of their parents (1881, 69-70). To maximise happiness in the next generation, those with a low capacity for happiness should not have children, concluded Edgeworth, and he commented favourably on Galton's notion of a refuge for the 'weak' in celibate monasteries (1881, 71-2).

Edgeworth did not take this idea any further:

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(3) Lest anyone be so foolish as to imagine that the proletariat had a large capacity to be happy, Edgeworth hastened to point out that 'the higher pleasures are on the whole most pleasurable ... those who are most apt to enjoy those pleasures tend to be most capable of happiness' (1881, 58).

it must be suspected that he was interested in eugenics only in so far as hereditarian ideas helped him in the production of a conservative utilitarianism. He worked on statistical theory from the 1880's onwards, but it was not along the lines of Galton's research programme. Edgeworth's general aim was the construction of a 'mathematical psychics' with two main subdivisions: the study of utility, which led him into his well-known work in mathematical economics; and the study of belief, which led him into research in statistical theory, in particular in those parts closely connected to the foundations of the subject, as in Edgeworth (1883a; 1883b; 1884; 1885; 1887). His work in this latter area was obscurely presented and had little impact at the time. Even those parts that might have been of use to other British statisticians, such as his work on the 'Edgeworth expansion' generalising the normal distribution or on the 'method of transformation', were not taken up.⁽⁴⁾ Only one statistician seems to have taken Edgeworth's work seriously enough to study it in detail: Arthur Lyon Bowley, for whom see section 5.4.

In the 1880's Edgeworth was the only person in Britain, with the exception of Galton, doing anything approaching serious and sustained general work in statistical theory/...

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(4) Welch (1958) points out that Edgeworth (1883b) provided a Bayesian solution to the problem, later made famous by Gosset, of making inferences about means from small samples. For Edgeworth this was, however, a completely theoretical problem, devoid of the practical context that was to give Gosset's work its force.

theory.⁽⁵⁾ Accordingly, Galton appears to have tried on more than one occasion to recruit Edgeworth to work on the statistics of heredity.⁽⁶⁾ In the early 1890's Edgeworth finally turned to a problem suggested by Galton's work, that of generalising the bivariate normal distribution constructed by Galton and Hamilton Dickson to an indefinite number of variables. Edgeworth's papers on correlation (1892a; 1892b; 1893a; 1893b; 1893c; 1893d) show clearly that he solved the problem in essence, even though they are marred by occasional errors, misprints and obscurities.⁽⁷⁾ Edgeworth, however, did no further statistical work along Galton's lines. Instead, in the words of Karl Pearson, he 'ploughed always right across the line of [the biometricians'] furrows' (quoted by Kendall, 1968, 262); Edgeworth thus stood aside from the main line of development of statistical theory in Britain.

The men so far considered all lived in more or less/...

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- (5) From Kendall and Doig (1968) I produced a list of papers published in Britain in the 1880's in the fields of statistical theory, error theory and actuarial theory (papers on narrow areas of probability theory, such as solutions to problems posed in Whitworth's Educational Times, were excluded). Edgeworth and Galton together accounted for a majority of these papers (40 out of 78).
- (6) See the Edgeworth-Galton correspondence in the Galton Papers (189 and 237).
- (7) This work has been the subject of some disagreement. Pearson (1920b) doubted whether Edgeworth should really be said to have solved the problem; Seal (1967) disagrees, arguing that Edgeworth showed in principle how to obtain the result. Once Edgeworth's determinantal notation is understood (it follows Salmon, 1859), what he was doing becomes clear; and Edgeworth (1893d), which was apparently overlooked by both Pearson and Seal, states explicitly how to construct an expression for the multivariate normal distribution.

less favourable circumstances and became fairly well known in their respective fields. Not so the next figure to be discussed, who published nothing in his lifetime and has so far escaped the attention of historians.⁽⁸⁾ Arthur Black (1851-93) was the son of David Black, solicitor and coroner in Brighton. David Black and his family are described by Garnett (1953, 4-6). Arthur was the eldest of eight children, of whom the sixth, Constance, later Constance Garnett, was to become famous for her translations of the novels of Tolstoy and Dostoyevsky. Little about Black's early life can be discovered from the available material. He took a B.Sc. degree of the University of London by private study, graduating in 1877. He must have attended some classes, for a letter from Weldon to Galton describes him as having been a 'favourite pupil' of W.K. Clifford, the famous Professor of Applied Mathematics at University College (4 June 1894; Galton Papers, University College, London, 340/C). Subsequently he earned a rather precarious living as/...

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- (8) I am indebted to Messrs David and Richard Garnett for providing me with information about Arthur Black, and for locating the surviving manuscript notebooks. Mr David Garnett has kindly allowed Black's notebooks to be placed in the Library of University College, London. Mrs Jacqueline Golden of University College, London kindly provided me with copies of letters from the Galton archive which refer to Black (293/A, 245/18A, 340/C). Professor Egon Pearson provided me with two further letters from the Karl Pearson archive, also at University College, London, and with the assistance of Miss Peek, Keeper of the Archives at Cambridge University, succeeded in discovering information on the abortive efforts to publish Black's Algebra.

as an army coach and tutor in Brighton, while pursuing his mathematical and philosophical interests. His marriage was fraught and unhappy, and he finally died by his own hand in very sad circumstances (The Times, 20 and 21 January 1893).

After his death his sister Constance and brother Robert, a doctor in Brighton, sought to publish some of his work. He left behind him a large, and apparently fairly complete, manuscript on the Algebra of Animal Evolution. This was sent to Karl Pearson, who was personally known to Constance; they moved in similar circles of radical intellectuals. Pearson started to read it, but realised immediately that it discussed topics very similar to those he was working on, and decided not to read it himself but to send it to Francis Galton for his advice. Galton was clearly impressed by it, and recommended its publication, although he admitted he found some of the mathematics rather difficult to follow. Cambridge University Press agreed to publish it, with Weldon acting as an editor. Problems seem to have arisen, however, in finding a mathematician to act as co-editor and finally all concerned agreed that part of the mathematical work should be extracted and published. M.J.M. Hill, Professor of Mathematics at University College, London, took responsibility for this. It would appear that Pearson continued to wish not to read the manuscript, so as to avoid being placed in a potentially difficult position. The resulting paper (Black, 1898) is the only published part of Black's work, and the reference to it in Kendall and Doig (1968)/...

(1968) appears to be the only reference to Black in the statistical literature.

Unfortunately, it has proved impossible to locate the manuscript of the Algebra of Animal Evolution. It was returned to Constance Garnett; a footnote in Black's paper describes it as being in her possession and available to be read by those interested. It was, however, not included in a set of some two dozen manuscript notebooks of Arthur Black in the possession of her son and grandson, Messrs David and Richard Garnett. However, the surviving notebooks, together with the material extracted from the Algebra by Hill, give some indication of the scope and nature of Arthur Black's work. Like Pearson and Weldon, Black was a convinced Darwinian. He took the side of scientific naturalism against its theological opponents. The main focus of his work seems to have been an attempt to use his considerable mathematical skills to develop a quantitative theory of evolution. One incomplete notebook, probably part of a draft of the introduction to his larger manuscript, is entitled:

An Algebra of Evolution, being an essay on the quantitative mathematical treatment of rate of change of specific types, as affected by severity of competition, extent of deviation from the average, longevity, fecundity, tendency to deteriorate, and pure chance.

Another notebook entitled The Theory of Deviation from an Average begins:

The subject of the following essay is the theory of the measurement and statistical treatment of individual variations, such as are exhibited by all/...

all individuals of all species of animals and plants from one another and from the average of their species. In particular the aim is to put the theory of variation of specific characters in course of time by natural selection upon a mathematical footing: the advantages of which will be to exhibit such parts of the theory as admit of proof in a demonstrative form, and to estimate quantitatively those tendencies to change which evolutionists describe. The treatment will be merely theoretical, that is the methods of dealing with such problems will be investigated, but the actual application of the results to special cases will not be entered upon. The data are probably not yet accumulated for that task. The theory itself dictates what kinds of data are needed.

It is impossible on the available evidence to give any assessment of how successful Black was in his overall task. The work extracted by Hill from the Algebra of Animal Evolution was an evaluation of the multiple integral

$$\int V \exp (-U) dx_1 dx_2 \dots dx_n,$$

'where U and V are homogeneous quadratic functions of the n variables x_1, \dots, x_n and a constant x_0 , and all the integrations are from $-\infty$ to $+\infty$, it being further supposed that U is essentially positive' (Black, 1898, 219). This is a very competent solution of a problem of some difficulty, but tells us little of the more statistical side of Black's work. On this the notebooks are more revealing.

Buried amongst a large bulk of unorganised material, nearly all of it rough working, are a couple of quite striking fragments. The first occurs during a discussion of problems to do with the probabilities of survival and reproduction. In investigating such problems Black naturally turned to the multinomial distribution and its properties. The most interesting/...

interesting aspect of this investigation is his discussion of an event which occurs n times where n is very large, and has r possible outcomes, with probabilities c_1, \dots, c_r .

Let z be the probability that outcome one will occur u_1 times, outcome two will occur u_2 times, and so on, with $u_1 + \dots + u_r = n$. Then z can be written, according to the

formula for the multinomial distribution, as

$$z = \frac{n!}{u_1! \dots u_r!} c_1^{u_1} \dots c_r^{u_r}$$

Now suppose that n can be expressed as the sum of r integers

U_1, \dots, U_r , where $U_1 = nc_1$ and so on. Let $v_1 = u_1 - U_1$,

and so on. If n is large, and v_1, \dots, v_r small compared to

\sqrt{n} , Black showed that z can be approximated by $k \exp(-\frac{1}{2}w)$,

where k is a constant and $w = v_1^2/u_1 + \dots + v_r^2/u_r$. This of course

is what would now be called the χ^2 approximation to the

multinomial distribution. Although Abbe and other authors

had discussed the χ^2 distribution previously, it had not

been from this standpoint (Pearson, 1931; Sheynin, 1966;

Sheynin, 1971a; Kendall, 1971). Only Bienaymé appears to

have approached the problem from the angle of the multinomial

distribution, and his analysis, although perhaps more

rigorous than Black's, did not lead to the final simple

expression (Lancaster, 1966). Of course, as Black did not

interpret the U_i as expected frequencies and the u_i as

observed frequencies, nor attempt to integrate his pro-

bability density z within a suitable contour, we cannot

credit him with anticipating Pearson's invention of the χ^2

goodness-of-fit test (Pearson, 1900c).

The/...

The second interesting fragment constitutes an apparently independent derivation of what is now called the Poisson distribution, though it perhaps should not strictly be attributed to Poisson (David, 1969; Sheynin, 1971b). This comes in a notebook entitled Problems relating to the Mathematical Treatment of Statistics: Periodicity and Deviation. Black obtained the distribution to give the probability of an incident occurring 0, 1, ... times in a given interval of time, when the average of its occurrence in a small unit interval has some small value, say Y. He showed that

... the rule is write certainty in the form $e^Y e^{-Y}$, and expand e^Y in powers of Y by the exponential theorem. The successive terms are the probabilities of 0, 1, ... incidents.

The surviving notebooks do not give any evidence that Black attempted to test the applicability of this distribution to empirical data.

If we include Karl Pearson, nine mathematicians who came into contact with Galton's work have now been considered.⁽⁹⁾ They responded in various ways, from giving Galton assistance on a specific problem to, in the case of Black and Pearson, deciding to devote the major part of their intellectual energies to statistical work along Galton's lines. The pattern of response does not seem explicable/...

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(9) Black never came into personal contact with Galton, and Pearson apparently did not until after he began work on biometric statistics. Nonetheless, it is clear that for both Galton's work formed a crucial exemplar.

explicable simply in terms of their occupational positions or institutional affiliations. A more important factor seems to have been the general attractiveness, or otherwise, of Galton's research programme. Pearson and Black appear to have been attracted by its political and cultural dimensions;⁽¹⁰⁾ the others, although in several cases not unsympathetic to Galton's aims, regarded Galton's work primarily as a source of interesting problems of a mathematical nature.

Given the fact that the only person aside from Galton who might be seen as a likely candidate to establish a school of statistical theory, F.Y. Edgeworth, did not in fact do so, this pattern becomes extremely interesting. It would suggest that the connections between statistical theory and eugenics found in the case of Galton affected not only his work but also the response to it, and thus played an important part in the early development of statistical theory as a specialty. There is little point in speculating what would have happened if Pearson had not been attracted to Galton's programme, or if Black had not killed himself. What can, however, be suggested is, first of all, that Galton's perception, quoted at the start of this section, that statistical theory was 'ripe' for systematic development/...

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(10) The evidence on Black's views is somewhat scanty. The notebooks suggest a man whose general intellectual position was similar to Pearson's, for example in his attitude to science; his sister Constance was a member of the Executive Committee of the Fabian Society, and Mr David Garnett suggests that Black himself was probably politically radical.

development by the early 1890's was correct; secondly, that examination of Galton's efforts at 'recruitment' indicates that what was needed was someone with a broad commitment to, and not merely a technical interest in, the field.

5.2 The Biometric School

In the previous section, what might be called the 'first phase' of the development in Britain of statistical theory as a specialty was discussed. That phase can be seen as ending around 1892, when Karl Pearson and W.F.R. Weldon began the collaboration that was to grow into the biometric school. A second phase of development, which continued until the 1920's, had started.

For the first phase of development, an analysis of individual cases was apt. Statistical theory in Britain did not have an institutional location. Such work as took place was done either by isolated individuals or by pairs of individuals in temporary collaboration. With the emergence of the biometric school, the focus of the analysis must shift. The institutionalisation of statistical theory had begun, and it is necessary first of all to understand the emerging institutional structure.

Happily, there is a considerable amount of secondary material to draw from here. Karl Pearson himself (1906/...

(1906; 1914-30, 3A), E.S. Pearson (1936-8) and, more recently, Lyndsay Farrall (1970, 54-202 and 318-25) have all discussed in some detail the development of the biometric school. It is unnecessary to repeat their descriptions. Instead, this section will focus on three particular aspects of the biometric school.

The first point emerges clearly from consideration of the development of statistical theory in Britain from almost any point of view: that is, the crucial role of the biometric school in this process. Its centrality can be seen from the fact that in the 1890's, the decade of its establishment, the members of this school were already producing around half the papers in statistical theory published in Britain.⁽¹¹⁾ From 1894, when Pearson began teaching his first advanced course in statistical theory, until the 1920's, when Fisher established an alternative centre at Rothamsted Experimental Station, the biometric school was the only institution in Britain providing an advanced training in statistical theory. Its importance is indicated by Ben-David's comment (1971, 151 fn.):

Those who actually taught [at University College] include 5 of the 15 persons named as the most important contributors to the development of present-day statistical method in the International Encyclopedia of Social Sciences.

Biometrika/...

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(11) A list of these papers, produced from Kendall and Doig (1968) in the same way as that referred to in the previous section, contained 122 items. Of these Galton, Pearson and their associates were responsible for 64; the next most important source was Edgeworth, who produced 13.

Biometrika (the 'house journal' of the biometric school) was for a long period the major publication outlet for work in statistical theory in Britain: it remains one of the world's foremost journals in this field.

It is clear, therefore, that in discussing the biometric school we are dealing with a social institution of prime importance in the development of statistical theory. A second important aspect of the school is, however, that in terms of financial and organisational backing statistical theory was essentially a subsidiary part of its activity. Two partly separate developments came together in the biometric school: a move from within the community of biological scientists to quantify biology, and the tradition of eugenically-orientated statistical work begun by Galton.⁽¹²⁾ The first development was crucial to the formation of the school; the second to its continuing existence and growth.

The move from within biology to quantify its subject matter can be traced to a crisis within the dominant tradition of professional evolutionary biology in Britain, the school of evolutionary morphology centred round F.M. Balfour at Cambridge. The aim of this school was to establish phylogenetic relations (evolutionary trees) between classes of organism by comparative study of their forms, relying in particular on the hypothesis that 'ontogeny recapitulates/...

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⁽¹²⁾ That these two aspects were intertwined is argued in chapter six.

recapitulates phylogeny¹. In 1886 one young member of the school, William Bateson, wrote:

Of late the attempt to arrange genealogical trees involving hypothetical groups has come to be the subject of some ridicule, perhaps deserved.
(Quoted by Provine, 1971, 37)

There seems to have been, at least amongst the younger practitioners of descriptive evolutionary morphology, a general openness to new, and hopefully more rigorous, methods of investigation.⁽¹³⁾

Galton's statistical studies, although focused on human heredity rather than general problems of evolutionary biology, offered a possible exemplar of just such a new method. W.F.R. Weldon (1860-1906) saw in Galton's work a way of reconstructing evolutionary biology on a sounder basis than that offered by the morphological approach in which he had been trained. Weldon, who in December 1890 became Professor of Zoology at University College, London, demonstrated in a series of four papers (1890; 1892; 1893; 1895) the applicability of Galton's methods to populations of crabs and shrimps. The first paper showed that measurements made on several local races of shrimp followed the normal distribution. This paper was sent to Galton to referee, and brought Weldon and Galton into personal contact, Galton aiding Weldon in revising the statistical analysis (Pearson/...

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(13) In terms of the overall development of biology, the most significant new approach was Roux's work in what he called Entwicklungsmechanik, the first parts of which were published in 1888 (Allen, 1975a, 21-8).

(Pearson, 1906, 282-3). The following two papers applied Galton's correlation techniques, using a non-graphical method of determining the coefficient of correlation devised by Weldon himself. Weldon (1895) attempted the ambitious task of demonstrating natural selection at work in a population of crabs. (14)

As outlined in chapter four, Weldon and Pearson began an active collaboration in the early 1890's. As Professors of Zoology and Applied Mathematics in University College, they were able to build up a small group of students and co-workers who were either independently supported or in posts associated with the two professorships. Thus, Pearson's first course on advanced statistics had an audience of two: George Udny Yule, Pearson's demonstrator, and Alice Lee, a lecturer in Bedford College. Weldon recruited several postgraduates, first at University College and then, from 1899, at Oxford; notable among these were Ernest Warren, Arthur Darbishire and Edgar Schuster. This group contributed to biometry and, in the case of the last named, eugenics. (15)

The work of Weldon and his postgraduates demonstrates the early importance of the move within the biological community to a statistical methodology. This move, however, largely petered out: the fact that it did not survive Weldon's/...

(14) See Norton (1973) for a discussion of this paper and of the reaction to it.

(15) See, for example, Warren (1902), Darbishire (1902-4), Schuster (n.d.).

Weldon's early death indicates its lack of implantation. Several possible causes can be adduced. It was work that required a relatively unusual combination of training; professional biologists were suspicious of the new methodology; the biometrician/Mendelian controversy may have led some biologists to identify biometric methods with hostility to Mendelism.⁽¹⁶⁾ Whatever the causes, biometry as a specialty within professional biology must be judged a failure.⁽¹⁷⁾

With the waning of biometry as a biological specialty, the overt connection between statistics and eugenics became of increasing importance in the development of the biometric school. From the early 1900's onwards, Pearson began the transformation of his still relatively haphazard and informal group into an established research institute. In this process, some resources were available to him simply through his university professorship and the general reputation of his work.⁽¹⁸⁾ Other funds, however, came specifically for eugenics. In February 1905, Francis Galton gave the University of London £1500 to establish a Eugenics Record Office, and from then until his death he gave/...

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(16) These last two factors are discussed in chapter six.

(17) See Farrall (1970, 186-9).

(18) Thus, from 1903 onwards he received through the University College authorities a grant of £500 per annum from the Worshipful Company of Drapers (Farrall, 1970, 129-31).

gave £500 per year for eugenics research (Farrall, 1970, 131). At the end of 1906, Galton asked Pearson to take over the direction of the Eugenics Record Office, which became known as the Galton Laboratory of National Eugenics (Farrall, 1970, 111). The Eugenics Laboratory, together with a 'Biometric Laboratory' established from Pearson's other resources and oriented more towards statistical theory as such, became the beginnings of a solid institutional base for the biometric school.

This base was further extended when Galton died in 1911. In his will he left the residue of his estate to the University of London for the establishment of a 'Galton Professorship of Eugenics' with 'a laboratory or office and library attached thereto', and recommended that the post be offered to Karl Pearson (K. Pearson, 1914-30, 3A, 437-8). A public appeal was launched for funds for a building for the Eugenics Laboratory, and supported in a Times leader in October 1911:

The state of morals and of intelligence disclosed by the recent strikes, the state of health of the rising industrial population as disclosed by the medical inspections of schools are alike in showing the need for the study and the application of Eugenics, and in affording support to the appeal which we bring before our readers.
(Quoted by E.S. Pearson, 1936-8, part 2, 190)

The appeal seems finally to have brought in some £2300.⁽¹⁹⁾ Most of the money was provided by friends and relatives of Galton/...

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(19) Calculated from the official subscription list in the Pearson Papers (247).

Galton and members of the Eugenics Education Society: there were no very large donations from businessmen.⁽²⁰⁾ A much larger sum of money was provided (though it was apparently not initially earmarked for eugenics) by a donation to University College from a businessman, Sir Herbert H. Bartlett.⁽²¹⁾

The money from Galton, Bartlett and the subscribers to the appeal fund made possible the provision of a building intended to house the Biometric and Eugenic Laboratories, which were now jointly called the Department of Applied Statistics, and enabled Pearson to give up his onerous teaching duties as Professor of Applied Mathematics and become Galton Professor of Eugenics. Thus, the first university department in Britain committed to advanced teaching and research in statistical theory was established, with the funds for its establishment coming largely from the connections between statistics and eugenics. Pearson had, at least in part, succeeded in securing the institutional base of biometric statistics.

It should be emphasised that the use by Pearson of the 'eugenic connection' to obtain support for statistical research/...

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(20) The Hon. Rupert Guinness and Lord Northcliffe both contributed, but only relatively small sums (£100 and £25).

(21) Pearson Papers (239). The money was passed to University College through the leading 'Liberal Imperialist', Lord Roseberry. Roseberry seems to have been sympathetic to eugenics, having previously promised £100 to the appeal fund (Pearson Papers, 238), and may have been responsible for 'steering' the money towards the Eugenics Laboratory.

research was not a cynical or opportunist strategy, but reflected both his personal beliefs on the relationship of eugenics and statistics and their actual coupling in the practice of the Biometric and Eugenic Laboratories. Pearson believed that eugenics had to have a statistical form to be properly scientific and a sound basis for social action (see chapter four and section 6.5): he was, for example, reluctant to become Professor of Eugenics unless allowed to carry on the direction of the Biometric Laboratory, with its programme of teaching and research in statistical theory (K. Pearson, 1914-30, 3A, 436). At the same time, as will be shown in detail in chapter seven, the needs of eugenics figured large in his work in statistical theory. In his last report to the Worshipful Company of Drapers, who had provided regular funds for the Biometric Laboratory, Pearson warned of the need to keep statistical theory 'in touch with practical needs' (E.S. Pearson, 1936-8, part 2, 230) and there is no doubt that in his mind eugenics - as 'the main, if not the sole, safeguard for future national progress' (K. Pearson, 1909d, 39) - was the source of the most central of these practical needs. In reality, there seems to have been little clear demarcation between the Biometric and Eugenic Laboratories, which shared personnel, methods and problems. The Laboratories are best seen as a unified research institute pursuing, at least in the period up to 1914, a multi-faceted but still integrated research programme.

The third aspect of the biometric school to be discussed is its internal social organisation. It would appear/...

appear that it was a coherent social group under the clear leadership of Pearson. Much of its work was collaborative. To the extent that the nature of this work tended to involve large numbers of measurements, and a very large amount of detailed arithmetical calculation, this was inevitable. Despite the division of labour involved, Pearson seems to have kept a close eye on the progress of work. He would frequently assist subordinates in the preparation of their work for publication. Ethel Elderton wrote that he 'always had time to sit down and discuss an individual problem. We did not go to his room, but he came round at least once a day to see everyone' (quoted by E.S. Pearson, 1936-8, part 2, 182). W.S. Gosset wrote:

... I gained a lot from his 'rounds': I remember in particular his supplying the missing link in the probable error of the mean paper - a paper for which he disclaimed any responsibility ... at 5 o'clock he would always come round with a cup of tea ... and expect us to carry on till about half past six. (E.S. Pearson, 1936-8, part 2, 182-3)

This social situation led naturally to a high degree of intellectual coherence, which was reinforced by the fact that the group possessed its own organs for publication (Biometrika, and the various series of Biometric and Eugenic Laboratory publications) over which Pearson appears to have exercised fairly direct control. Indeed, Yule claims that Biometrika was 'surely the most personally edited journal that was ever published' (Yule, 1936, 100).

Within the group strong personal ties were formed, and a considerable esprit de corps seems to have existed. Yule/...

Yule writes that, 'in the old days', Pearson and he 'spent several holidays together' (1936, 101). When Weldon moved to Oxford, the biometricians would meet in a country cottage for a working weekend (Pearson, 1906, 309-10). Yule informs us that there was much social intercourse between Pearson and his students, and that '... the influence of [Pearson's] striking and dominating personality went far beyond the class-room walls' (1936, 100).

There seems to have been a strong sense of the correctness of the scientific approach of the biometric school, and conversely of the weakness of much of the work done outside it. In Karl Pearson's lectures, writes Egon Pearson, 'we were told of the sins of many people' (1936-8, part 2, 207). Pearson was a fierce controversialist, and on occasion personally cold and hostile to those with whom he disagreed. This attitude does not seem to have sprung from psychological disposition: Yule, who had personally felt Pearson's anger, conceded that Pearson was in non-intellectual matters unfailingly courteous and friendly (Yule, 1936, 101). Perhaps it makes more sense to see Pearson's attitude as the response of the man at the centre of a small group of researchers, pursuing what he felt to be work of the greatest scientific, social and moral importance in a world he interpreted as prejudiced, indifferent and hostile. In any case, the consequence was a further tightening of the group boundary. Those members, or former members, of the biometric group who espoused what Pearson/...

Pearson considered to be error were cut off from the group. As the letters between Yule and Greenwood in the period immediately before 1914 indicate, those 'expelled' had a definite sense of a bounded group from which they had been excluded (Yule Papers, box 1; Yule-Greenwood Letters).

5.3 The Members of the Biometric School

A large number of people worked, at least for a short period, in the Biometric and Eugenic Laboratories at University College. From 1911 onwards the Laboratories had a joint staff of between six and twelve (Farrall, 1970, 320-1). In addition to paid staff, postgraduate students and other individuals with their own sources of finance came to the Laboratories to study and do research. Over 40 individuals are known to have worked with Pearson in the period 1900 to 1914.⁽²²⁾

On the available evidence, it is impossible to tell why/...

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- (22) The following list, which does not claim to be exhaustive, was compiled from the employees of the Laboratories as listed by Farrall (1970, 320-1), together with those who collaborated with Pearson in his published work: Ethel Elderton, W.P. Elderton, P.F. Everitt, David Heron, W.S. Gosset, J.A. Harris, C.B. Goring, John Blakeman, W.R. MacDonnell, Raymond Pearl, Leon Isserlis, E.C. Snow, H.E. Soper and Major Greenwood, all of whom did work requiring considerable knowledge of statistical theory; and A. Fry, A.O. Powys, M. Lewenz, F.E. Cave-Browne-Cave, S. Jacob, M. Radford, A. Barrington, G. Uchida, A. Wright, E. Pope, Julia Bell, E.Y. Thomson, H.G. Jones, E.H. Nettleship, C.H. Usher, W. Gilby, R. Crewdson Bennington, E. Lea-Smith, M. Crawford, H. Rishbeth, Edith M.M. de G. Lamotte, H.J. Laski, G. McMullan, K.V. Ryley, M.H. Williams, B. Cave, G. Jaederholm and A. Davin, all of whom contributed to other aspects of the work of the Laboratories.

why these individuals came to work with Pearson. Some, such as the future social-democratic political theorist, H.J. Laski, appear to have come because of their enthusiasm for eugenics.⁽²³⁾ Others had no interest in eugenics, but came to learn specific skills that would be useful to them in other contexts. W.S. Gosset, to be discussed in chapter eight, is a case in point. In most cases, however, the motives of the individuals concerned are quite unknown.

More information is available on the subsequent careers of those who passed through the Laboratories. Two main career paths seem to have been followed by those who, in their time at the Laboratories, became sufficiently skilled in statistics to engage in independent publication. A minority became full-time eugenic or biometric researchers, or took up teaching and research in statistical theory. Some of these, notably Ethel Elderton, obtained permanent employment at University College. The rest found employment elsewhere. Raymond Pearl and J.A. Harris returned to academic careers in biometry in the United States, while Greenwood and Soper went on to statistical careers in Britain. The majority of those trained by Pearson, however, found employment outside academic research and teaching or in non-statistical academic work. Heron became chief statistician to the London Guarantee and Accident Co. Ltd. (E.S. Pearson, 1970a). Edgar Schuster eventually became Assistant Secretary to the Medical Research Committee, forerunner of

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(23) Martin (1953, 14-16); K. Pearson (1914-30, 3B, 606-9); H.J. Laski (1912).

the Medical Research Council (Paton and Phillips, 1973). Leon Isserlis became statistician to the Chamber of Shipping (Irwin, 1966). E.C. Snow became Director of the United Tanners Federation (G.R. White, 1960). W.S. Gosset returned to the Guinness Brewery in Dublin. W.P. Elderton never left employment as an actuary (Menzler, 1962). John Blakeman became head of the Mathematics Department of Leicester College of Technology and subsequently Principal of Northampton College of Technology (Who was Who, 1914-30).

Employment opportunities did then exist for Pearson's highly trained students. These opportunities were, however, not such as to permit the easy diffusion of the particular type of eugenically-oriented statistical theory pursued at University College. Finding employment nearly always meant turning to other kinds of work. Thus, Pearson complained in a letter to Galton in 1909:

You must remember that at present the training in statistics does not lead to paid positions. It is beginning to, but the posts available are few ... In the last four or five years I have had at least two or three really strong men pass through my hands, but I could not frankly say: 'Stick to statistics and throw up medicine or biology because there is some day a prize to be had'. I feel sure, however, with a future, such men will naturally turn to Eugenics work. Only this last winter one of my American students said: 'I wish I could go in for Eugenics, but my bread and butter lies in doing botanical work. I know that definite posts are there available'. And that was precisely the case with Raymond Pearl, who has now got the control of an Agricultural State Breeding Station - he was far keener on man than on pigs and poultry, but the public yet has not realised that it needs breeding also! ... At present the biometrician is the man who by calling is medical, botanical or zoological, and he dare not devote all his enthusiasm and energy to our work. The powers that be are against him in this country.

(K. Pearson, 1914-30, 3A, 381)

Let/...

Let us consider one case in detail, that of Major Greenwood (1880-1949). One individual cannot, of course, be claimed necessarily to be typical, but in Greenwood's case there happens to be more information available than on almost any of the other students of Karl Pearson (apart, that is, from Yule and Gosset who are considered in chapters seven and eight). In addition, Greenwood was one of the most important of Pearson's pupils from the point of view of the future development of statistics in Britain.

A typewritten autobiographical note in the Pearl Papers (filed with Greenwood to Pearl, 4 April 1926) gives us some insight into Greenwood's background and the reasons why he started work with Pearson. His father and grandfather shared a medical practice in the East End of London.

On the whole my childhood was pretty typical of a London lower middle class ménage. It had a good deal of the ugly snobbishness that H.G. Wells has gibbeted, but there was a real love of books and a real knowledge of books.

Greenwood's father was determined that he should follow the family tradition and become a doctor, but he lacked enthusiasm for this. Nevertheless, at age 18 he entered the London Hospital, 'the great hospital of East London'. 'I continued to loaf, until Pearson's Grammar of Science did for me what it had done for other lads and I found my intellectual interest.' That was in 1901, a year after the publication of the second edition of the Grammar containing a summary of the results of the first decade of biometric work. Greenwood confessed that for 'Karl Pearson, I developed/...

developed an almost school-girl passion' (Hogben, 1950, 140). In the intervals in his medical curriculum Greenwood worked on a biometric study, contrasting healthy and diseased organs, which was published in Biometrika (Greenwood, 1904). The conclusion of the paper (1904, 73) was that, on the theory of natural selection,

... death before senility as far as it is selective is the destruction of the less fit, i.e. of those not approaching within certain limits the type suitable to the environment. Thus it comes about that we shall expect on the Darwinian theory to find the individuals who die of disease in adult life to be more variable and less highly correlated in their organs than the 'healthy'. This is precisely what we do find...

In 1904 he graduated in medicine. His autobiographical note continues:

My father allowed me to go to Pearson for a year, provided that at the same time I was assistant in his practice (which was a very easy one). That year, 1904-5, was a year of furious work. Pearson's assistant Blakeman came to board at my father's house (my mother died at the end of November 1904). He was about my age and a Cambridge Wrangler. His effortless superiority in mathematics and the difficulty of following Pearson's lectures stimulated my vanity and I have never worked so furiously as I did in that year. Of course I did not become a mathematician; I am not one now, I have not the temperament, but I laid a foundation upon which I have built not altogether inefficiently. At the end of the year, I had another piece of luck. Hill [Leonard Hill, the physiologist] asked me to be his demonstrator and the British Medical Association gave me a research scholarship. So at the age of 25 I entered the profession of scientific research.

Thus, it would appear that Greenwood was won to statistics by his attraction to the general aspects of Pearson's programme. He adopted many of Pearson's controversial views, for example on the importance of hereditary factors/...

factors in the incidence of tuberculosis:

... in the present state of knowledge, it is difficult to believe that the parental correlation of between 0.46 and 0.68 for pulmonary tuberculosis is not a measure of inherited predisposition rather than of parental infection.
(Greenwood, 1909, 267)

In a paper published in the Eugenics Review (Greenwood, 1912, 289-90) he supported the eugenicists' argument on the existence of a hereditary type 'physiologically inferior to the normal' comprising 'the tuberculous, the criminal, the mentally ill-balanced', and so on.

In 1910 Greenwood was appointed statistician at the Lister Institute of Preventive Medicine and, as he wrote in his autobiographical sketch, 'became a statistician for good'. Later he became the first Senior Statistical Officer in the Ministry of Health and, in 1927, first Professor of Epidemiology and Medical Statistics at the London School of Hygiene (Hogben, 1950, 142). Greenwood played a major part in the development of medical statistics. Hogben concludes (1950, 141):

That statistical methods are now beginning to enlist the respect of the medical profession, is due in no small measure to Greenwood's pioneer work on large-scale trials to assess the efficacy of prophylactic and therapeutic measures.

It is interesting to note that Greenwood's commitment to eugenics did not long survive his entry into the new role of statistician working in epidemiology and preventive medicine. It seems likely that in an occupational setting concerned with environmental measures to prevent the spread of disease, he found the pessimism of the eugenicists, for/...

for example about the campaign against tuberculosis, unsupportable. His letters to his close friend Yule reveal his growing doubts. Thus, he wrote to Yule on 30 June 1913 about the biometricians' neglect of the environment:

All this chatter about nutrition having no relation to, not the Anlage of intelligence - that is something we know nothing about - but the manifestation of the Anlage as shown in the shaping of the child at school either in work or in the impression he produces on the teacher is manifest balderdash. Give a dog a protein-free diet and he will become a corpse after a certain number of days, give him protein but not enough to keep him in nitrogenous equilibrium and he will equally become a corpse in a rather greater number of days. Now we know that many of the kids are not in nitrogenous equilibrium (Rowntree etc. ad nauseam). All this is not just medical dogma but hard solid experimental fact. Really if this is all we statisticians can do towards the solution of social problems ...
(Yule Papers, box 1)

By 1914, he was prepared to join with Yule in publicly attacking some of Karl Pearson's published work in eugenics (Greenwood and Yule, 1914).

The sequence in the case of Greenwood appears to have been as follows: recruitment into the biometric school, based at least in part on the general attractiveness of Pearson's programme; the learning of skills and the use of these both in applied work in a broadly eugenic framework and in theoretical work in statistics (for an example of the latter, see Greenwood, 1913); departure from the immediate context of the biometric school in order to obtain employment; growing doubts, perhaps stimulated by the new occupational setting, about the validity of eugenics; application of the skills learnt for different purposes. This/...

This sequence was, of course, not universal. Individuals could join the school for purely instrumental reasons, and not adopt eugenic beliefs. Others, like Greenwood, had to leave to find a job, but may, unlike him, have retained eugenic beliefs: a case in point seems to have been David Heron (see Heron, 1919). This is as we might expect. Any institution can expect to attract to itself individuals with a variety of motives, many divergent from the initial goals of the institution. The institution will not 'stamp' those who pass through it with a set of attitudes for all time. Some will retain attitudes learnt in the institution while others will not, and the explanation of these differences will have to be sought, in part, in the contingencies of their future careers.

5.4 Statistics outside the Biometric School

On almost any indicator one cares to choose (for example, quantity of publications, number of researchers, importance of results obtained, or role played in recruitment and training), the biometric school dominated statistical theory in Britain in the period from the early 1890's to 1914. Nevertheless, there were individual statistical theorists who were not members of the biometric school, and these also have to be considered.

Perhaps the single most important point about them is that they were indeed individuals. There was no other coherent/...

coherent group of workers to rival the biometric school. Edgeworth, the most distinguished statistician of the older generation not trained in or associated with the biometric school, remained an effective isolate. His sole statistical follower, Arthur Lyon Bowley (1869-1957), lectured on statistics at the London School of Economics, and in 1915 was appointed Professor of Statistics there (A.H. Bowley, 1972, 81). Important though Bowley's work in social statistics and econometrics was, he did not develop a school of statistical theory.⁽²⁴⁾ Within psychology, a number of workers turned to 'psychometrics', notably Charles Spearman, Cyril Burt and Godfrey Thomson. Again, their work, while important within psychology, was not productive of much innovation within statistical theory: from the statistical point of view it can indeed be seen as largely derivative of that of Galton and Pearson. A number of other individuals became interested in statistical theory from a wide variety of viewpoints, such as John Brownlee (medical statistics and perhaps eugenics) and John Maynard Keynes (probability in relation to philosophy and logic).

Among those who were not members of the biometric school but who developed an interest in statistical theory in the period 1900 to 1914, one individual of course stands out: R.A. Fisher. Fisher was to dominate British statistics from/...

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(24) For Bowley, see A.H. Bowley (1972) and Allen and George (1957).

from the late 1920's to the 1950's to almost the same extent as Karl Pearson dominated the statistics of the previous 30 years. Fisher's work in biology and statistical theory will be discussed below, in chapters six and eight. In the context of this chapter, only one question will be asked about him. Why did he become a statistician?

Fisher was educated at Stanmore Park School and Harrow, and in the Autumn of 1909 went on a scholarship to Gonville and Caius College, Cambridge.⁽²⁵⁾ Within two years he had developed a strong interest in statistics, which he maintained despite not finding a permanent post in the field of statistics until 1919.

Statistical theory as such was not taught at Cambridge until 1912, when Yule was appointed to a lectureship in statistics. By that time, Fisher's interest in statistics/...

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(25) Details of Fisher's early life are based on Mahalanobis (1938), which was itself based on information given to Mahalanobis by Fisher (F. Yates, personal communication). Subsequent biographical notices (such as Yates and Mather, 1963) rely on the Mahalanobis account for the pre-1920 period. It should be noted, however, that the Gosset-Fisher-Pearson correspondence published by E.S. Pearson (1968) has shown this account to contain certain inaccuracies, and it should therefore be read in conjunction with this correspondence and that published by McMullen (ed.) (n.d.).

Fisher's papers are in the care of the Genetics Department, University of Adelaide, and I was unable to obtain access to them. The records of the Cambridge University Eugenics Society, which I found in the Library of the Eugenics Society in London, make possible some supplementation of the published record. In addition, several colleagues and students of Fisher kindly allowed me to interview them: F. Yates, D. Finney, W. Federer, G. Wilkinson and D. Hayman. Fisher's daughter, Mrs Ruth Box, who is working on a biography of Fisher, had the kindness to reply to several written questions.

statistics was already developed, and in any case he seems to have attended only one of Yule's lectures. A more possible source of Fisher's interest was his undergraduate tutor, the astronomer F.J.M. Stratton (1881-1960). Stratton was well versed in the theory of errors, and he lectured in the subject.⁽²⁶⁾ This was, of course, not unusual for an astronomer. Stratton, however, was at least temporarily attracted to the idea of applying error theory outside astronomy. He teamed up with the agriculturalist T.B. Wood to write a paper (Wood and Stratton, 1910) advocating the use of error theory techniques in agricultural research:

The astronomer, being a mathematician, has devised a method of estimating the accuracy of his averages, which he invariably applies with great advantage. The agriculturalist cannot do better than follow his example. By doing so he will often be prevented from publishing experimental results which can only be misleading to those who read and act on them. The method consists in finding the 'probable error' of a result by the device known as 'least squares'. (Wood and Stratton, 1910, 425)

Stratton published one other non-astronomical scientific paper (Stratton and Compton, 1910). This concerned right-handedness and left-handedness in barley (barley leaves are frequently folded so that one margin overlaps the other) and in man. Stratton helped Compton analyse the barley data, and together they fitted to the human data a simple Mendelian model modified by a certain percentage of 'accidental' change/...

(27) Fisher's role in this is not mentioned by any of his biographers. Although Mahalanobis states that

(26) Brunt (1917) was largely based on Stratton's lectures. I owe this reference to Edwards (1974, 14). For Stratton's own use of the theory of errors, see, for example, Stratton (1909). For details of Stratton's career, see Chadwick (1961).

(28) Fisher (1971) has now been published by Norton and Pearson (1976).

change (by which genetically left-handed individuals in fact become right-handed, etc.).

Stratton may have played an important role, then, in generating Fisher's interest in statistics, and in teaching him the established body of error theory techniques. After graduating in 1912, Fisher spent a further year on a physics scholarship, studying error theory with Stratton and statistical mechanics with James Jeans. It is, however, far from certain that this kind of exposure to statistics would have generated in Fisher a desire to begin serious work in statistical theory. In the purely academic context, there was little incentive to do research in statistical theory, as is evidenced by Stratton's own return to purely astronomical work. A non-academic stimulus may well have been necessary.

Just such a stimulus was provided by Fisher's involvement in the Cambridge University Eugenics Society.⁽²⁷⁾ The records of this society contain three unpublished papers by Fisher (1911; 1912a; 1912b).⁽²⁸⁾ Fisher and a fellow undergraduate, C.S. Stock, appear to have been the main force behind the Society, which was founded in the Spring of 1911. Although/...

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(27) Fisher's role in this Society is not mentioned by any of his biographers. Although Mahalanobis states that 'the object of the statistical theory of evolution is to supply a sound basis for eugenics, the science of man' (1938, 244), later writers on Fisher have tended to play down his eugenic concerns.

(28) Fisher (1911) has now been published by Norton and Pearson (1976).

Although it attracted such influential patrons as Lord Rayleigh and the Bishop of Ely, and a wide ranging academic membership (including J.M. Keynes), the senior members of the Society seem to have played little active part in it. '[We] see so little of them, hear so little from them', complained Fisher (1912b). The most regular activity of the Society was the series of discussion meetings held by its undergraduate group. These meetings began in Fisher's rooms in October 1911, when Stock gave a general introduction to 'The Eugenic Field'. At the second meeting on 10 November 1911, Fisher introduced the group to the scientific basis of eugenics, with a paper entitled 'Heredity, comparing the Methods of Biometry and Mendelism' (Fisher, 1911). This paper shows that Fisher had already immersed himself in the academic literature relevant to eugenics. He had clearly read widely in the two major competing approaches to heredity, and had thought deeply and in an original fashion about the difficult topic of multifactorial Mendelian models.

The Society itself was shortlived. Its activities seem to have ceased by the outbreak of the First World War (it was revived after the War, but finally ceased to exist in 1923, according to a pencilled note in the file of records). However, the effect on Fisher of his involvement with eugenics was much more long-lasting. Around this time, he read 'carefully and very critically' Pearson's series of 'Mathematical Contributions to the Theory of Evolution' (Mahalanobis/...

(Mahalanobis, 1938, 240), and he began original work in statistical theory. It seems unlikely that Fisher would have done this if it had not been for the stimulus of eugenics. During the period 1913 to 1919, when he moved around restlessly from job to job, the Eugenics Society (of which he quickly became a leading member at the national level) seems to have played an important role in encouraging him in his work. In a paper in the Eugenics Review, he concluded a simplified outline of his now famous work on 'The Correlation of Relatives on the Supposition of Mendelian Inheritance' by acknowledging his

... deep sense of gratitude to the Eugenics Education Society, who have most generously assisted me throughout; and in particular to Major Leonard Darwin whose continual kindness and encouragement has enabled me to carry through the work.

(Fisher, 1918b, 220)

Fisher dedicated his Genetical Theory of Natural Selection (1930) to Leonard Darwin (who was the youngest son of Charles Darwin and President of the Eugenics Society for many years), 'in gratitude for the encouragement, given to the author, during the last fifteen years, by discussing many of the problems dealt with in this book'. (29)

It/...

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(29) Given the untechnical nature of Leonard Darwin's publications (for example, L. Darwin, 1926) it would seem likely that his help was in the nature of general support rather than detailed advice. I have not been able to find any extant correspondence between Leonard Darwin and Fisher. Some of Leonard Darwin's correspondence is at Down House; by courtesy of Roy MacLeod I was able to see another set of letters of Leonard Darwin's that was being catalogued at Sussex University and is now in Cambridge University Library.

It is possible, then, to conclude that Fisher's commitment to eugenics may have played a major role in motivating and sustaining his early interest in statistical theory and statistical biology. It seems to have been largely in the context of the Cambridge University Eugenics Society that his early studies of these areas were made, and the Eugenics Education Society appears to have been an important reference group for his work, encouraging him in it and providing him with an important vehicle for the publication and discussion of his ideas. Thus, consideration of the case of Fisher supports the case that the connection between statistics and eugenics was important to the development of the specialty of statistical theory in Britain.

5.5 From Eugenics to Statistics

In this chapter I have analysed the scientific 'party' founded by Galton and Pearson. Previous chapters have shown that eugenics and social Darwinism played a major role in the motivation of these two men to found this 'party'. Its subsequent fate also was closely tied to these motivating concerns. The general attractiveness of eugenics played at least a partial role in attracting several key individuals to work in statistics, most notably R.A. Fisher. The establishment of the Department of Applied Statistics at University College would have been impossible without money which was given by Galton and others for eugenic research. The connection between statistics and eugenics/...

eugenics thus in large part accounts for the partial institutionalisation of statistical theory as a scientific specialty that took place in Britain prior to 1914.

To leave the story there would, however, be misleading. Techniques were being developed by Galton, Pearson and their followers that were of much wider potential application than merely to eugenics. The skills of the staff of the Biometric and Eugenic Laboratories could be turned to other fields. During the First World War, the resources of the Laboratories were largely devoted to work on employment statistics and ballistics: a development Pearson the patriot welcomed, but Pearson the eugenicist found frustrating (see E.S. Pearson, 1936-8, part 2, 241-5). Although the Department of Applied Statistics had grown out of work on eugenics and evolution, the connection was not unbreakable.

The War was the turning point. Egon Pearson writes:

... in the early summer of 1914 the auspices for the future of biometry and eugenics were good... A spacious new building was nearing completion... funds for its equipment were in the bank ... Courses of public lectures were well attended; though sometimes hidden behind a screen of controversy and of journalistic popularisation of the concept of eugenics, a growing body of opinion was learning to appreciate the value of statistical method.
(1936-8, part 2, 195)

During the War, inflation ate away the Department's funds, its research programme was interrupted, and Pearson found it difficult to retain trained staff as new openings for statisticians/...

statisticians opened up in government service. Just as seriously, by the end of the War the enthusiasm for eugenics which characterised the years before 1914 had largely passed. Possible causes of this are discussed in chapter three; the consequences for Pearson's group were that the unified research programme pursued prior to 1914 began to fragment. As eugenics became less important, the various parts of the Pearsonian programme - statistical theory, anthropometry, craniometry, and so on - began to develop more independently. For example, when Karl Pearson's son Egon Pearson was appointed to a job in the Department in 1921, his line of work came to diverge more and more from the statistical theory which Karl Pearson had developed in integral connection with eugenics and Darwinism.

Karl Pearson fought against the tide. Despite, for example, discouragingly poor audiences at public lectures on eugenics, Pearson established in 1925 a new journal, Annals of Eugenics, devoted 'wholly to the scientific treatment of racial problems in man' (E.S. Pearson, 1936-8, part 2, 217). The journal survives to the present, though under the title Annals of Human Genetics. On Pearson's retirement in 1933 there came, however, the hardest blow. The authorities of University College decided to divide his Department of Applied Statistics into two, establishing separate chairs of Statistics (to which Egon Pearson was appointed) and Eugenics (to which R.A. Fisher was appointed). A legacy from W.F.R. Weldon's widow made possible the establishment/...

establishment of a chair of Biometry (to which J.B.S. Haldane was appointed). Despite this increase in the number of senior posts, Pearson felt that the division of his Department constituted a fragmentation which negated his life's work, and he bitterly opposed it (E.S. Pearson, 1936-8, part 2, 231-2).

So the unification of statistics, eugenics and biometry in Pearson's programme did not survive. Eugenics in the academic context became human genetics, largely (though perhaps not entirely) lacking the political thrust of Pearson's eugenics. New factors came to be of importance in the development of statistical theory (some of these are discussed in chapter eight). Yet the statistical techniques of the biometric school were, in many cases, integrated, albeit in a changed interpretation, into the new statistical theory. The Department created by Pearson survived, although it was divided. Pearson's students carved out careers for themselves in these changed circumstances. Perhaps the best way to conceptualise this process is, as suggested at the beginning of this chapter, in terms of the formation of side bets. The aim of Pearson and, before him, Galton, had been to create a scientific specialty and a research institute in which statistical research into heredity and evolution would be pursued, with the ultimate aim of the application of the knowledge gained in a eugenic programme. In pursuing this aim, they had recruited others to this programme, funded and established a new University department/...

department, and so on. But in doing so, interests, individuals and bodies extraneous to the initial aim became involved. The most systematic and advanced training then available in mathematical statistics was offered. It attracted those who had no interest in eugenics as such, and took on a momentum of its own. To give the research institute a stable setting, it was established within University College: this committed the College authorities to it, but gave them power over it. These, and other similar side bets, meant that the institutional development started by Pearson and Galton was no longer tied to their initial purposes alone. As eugenics waned, the side bets became more prominent until they came to dominate the initial purposes.

Chapter Six

Biometrician versus Mendelian

The best-known of the controversies involving Karl Pearson and his co-workers is that with the early Mendelian geneticists led by William Bateson. It was marked by the shattering of personal friendships, by heated public debate, by suggestions of fraud and by long-standing divisions within the British scientific community. Pearson suggested that the early death of his co-worker Weldon could be attributed in part to the strain of the controversy (1906, 311). Few scientific controversies rival the biometrician/Mendelian dispute in terms of contemporary public prominence, or in terms of the later attention of historians. The controversy has been the subject of recent studies by Froggatt and Nevin (1971a; 1971b), Provine (1971), Cock (1973), Norton (1973; 1975a; 1975b), de Marrais (1974) and Farrall (1975). The course of the controversy has, therefore, been thoroughly documented, and various suggestions for explanations of it put forward. The methodological issues raised by these are discussed in MacKenzie and Barnes (1975). In this chapter, I shall not discuss the detailed course of the controversy (for which see the above studies and MacKenzie and Barnes, 1975, 165-73). I shall attempt instead to provide an explanatory/...

explanatory account of the controversy, using the perspective on Karl Pearson developed above, together with additional material on William Bateson.⁽¹⁾ The chapter will conclude with a brief discussion of the work of R.A. Fisher, who, together with J.B.S. Haldane and Sewall Wright, is regarded as having resolved the controversy, and, in doing so, as having created modern population genetics (Provine, 1971, especially 130-78).

6.1 Green Peas, Yellow Peas and Greenish-Yellow Peas

In 1900, Mendel's work on heredity was 'rediscovered' by three Continental biologists, Hugo de Vries, Carl Correns and Erich von Tschermak.⁽²⁾ The Cambridge biologist William Bateson (1861-1926) seized eagerly on the new approach. He became the leading British Mendelian, and played a crucial role in developing the new 'paradigm' and extending it into different fields.⁽³⁾ He coined the term 'genetics', and the new discipline it refers to owed a great deal to his work. Much of the terminology of Mendelian genetics is his (for example, 'homozygote' and 'heterozygote/...

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(1) This account is based on that of MacKenzie and Barnes (1975), but has, I hope, been strengthened.

(2) I place the word 'rediscovered' in inverted commas because of the extremely interesting suggestion by R.C. Olby (1975) that the 'rediscoverers' read into Mendel's work what was not in fact there: a theory of genetic determinants in the modern sense.

(3) For Bateson's life and work see B. Bateson (ed.) (1928) and W. Coleman (1970).

'heterozygote'), and many early examples of the successful use of Mendelian explanations are to be found in his work and that of his group of co-workers, of whom R.C. Punnett (1875-1967) was the most prominent. Karl Pearson and W.F.R. Weldon, on the other hand, responded to Mendelism negatively. Pearson described Mendelism as a largely unproven theory as late as 1930 (1914-30, 3A, 288), long after its virtually total acceptance by professional biologists. He continued to support a different approach to the study of heredity, based on his own early work and that of Francis Galton before him.

Bateson and the Mendelians operated with a theoretical model of the process of heredity, at the basis of which were discrete, elementary genetic factors. These latter we have come to call 'genes', but that term is somewhat misleading because we tend to think of the gene as a physical thing, while at the beginning of the period discussed here the Mendelian factor was a purely theoretical entity. William Bateson, for example, never fully accepted the notion of the Mendelian factor as a material particle and disliked the chromosome theory on which this imputation was based (Coleman, 1970). Mendelian factors were held to pass unchanged from parent to offspring: pairs of factors underwent segregation and random distribution, but no blending of factors took place. Using elementary probability theory, together with assumptions about, for example, the dominance of one factor over another in the visible/...

visible manifestation of the factors in the offspring, theoretical accounts of processes of heredity could be produced. These accounts were applied to the inheritance of characteristics such as, classically, the green and yellow colourations, and smooth and wrinkled forms, of pea seeds.

The biometricians, on the other hand, did not use a developed, explicitly theoretical model of heredity.⁽⁴⁾ If we were to seek a single exemplar as characteristic of their approach, it would be the treatment of quantitative, easily measured characteristics such as height. Galton's 'typical laws of heredity' (1877) were descriptions of statistical regularities in the relationship between parental and offspring characteristics. Pearson (1896, 259) formalised this approach with his operational definition of heredity as the correlation between parental and offspring characteristics. The concept of heredity predominant in the work of the biometric school was thus, to use the modern terminology, that of phenotypic resemblance.

These two different approaches to heredity can be seen as 'incommensurable' (Feyerabend, 1962; Kuhn, 1970). For the Mendelians, the prime task was the development of a model of the process of heredity. Particular uses of the model/...

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(4) Weldon did attempt to develop a model, wider in scope than the Mendelian model, that would account for phenomena such as segregation and dominance as well as for blending inheritance. This was never published in his lifetime, but is summarised in Pearson (1908): the development of Weldon's ideas can be traced in his letters to Galton from 1900 to 1906 (Galton Papers, 340 G-J).

model certainly had to be checked against the empirical data produced by breeding experiments, but a failure in any given case would not lead to the discarding of the basic model. Rather, an attempt would typically be made to elaborate the model further, for example by the development of concepts such as partial dominance, so as to explain successfully the puzzling phenomena. More difficult puzzles, such as the problems in providing a simple Mendelian explanation of the inheritance of characteristics such as human height, were simply set aside for later consideration. For the biometricians, on the other hand, the task was primarily one of the description of phenotypic resemblance. 'Mendelism is only a truth so long as it is an effective description', wrote Pearson (1914-30, 3A, 288). The simplicity of early Mendelism was a point against it, not for it. What appears to be Pearson's earliest discussion of Mendelism⁽⁵⁾ considered Mendelism as a description of phenotypic resemblances, and concluded that it was unlikely to fit all the cases of inheritance of characteristics such as eye-colour and coat-colour, much less more complex characteristics.⁽⁶⁾

So the biometricians and Mendelians differed in their/...

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- (5) An undated manuscript entitled 'Mendel's Law', a copy of which was kindly sent to me by Dr Maxine Merrington of University College, London.
- (6) If Olby's view of Mendel's papers is justified, then Pearson, in considering Mendelism a theory of phenotypic resemblances and not of genetic determinants, was reading them correctly, and the early Mendelians reading them incorrectly!

their concepts of heredity (phenotypic resemblance versus a process of passage from parent to offspring of theoretically posited factors). Their criteria of scientific evaluation differed correspondingly. Phenomena for which successful Mendelian explanations could not be found were, for the Mendelians, puzzles to be resolved; for the biometricians, they were arguments against Mendelism. This incommensurability extended to accounts of particular phenomena. Mendel's experiments were predicated on the unproblematic classification of peas into different classes (yellow/green; smooth/wrinkled). He deliberately used only characteristics which he felt to 'permit of a sharp and certain separation' (1865, 45). But the biometricians doubted that this sharp differentiation was possible, even for the characteristics that Mendel had chosen. Weldon (1902) argued that pea seeds did not fall naturally into Mendel's classes, but shaded gradually from yellow to green through intermediate tones, and from smooth to wrinkled by various degrees. He presented photographs of pea seeds to prove his point.⁽⁷⁾ In reply Bateson argued that Weldon had used a 'mongrel' pea, rather than the 'pure' variety needed to demonstrate Mendelian phenomena (1902, 188-9). But the very notion of the 'purity/...

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- (7) The colour plate illustrating Weldon's article caused much concern because, through technical difficulties in colour reproduction, it at first showed half the pea seeds green, and half yellow, instead of the continuous gradation of colour that Pearson and Weldon felt undermined Mendel's approach. Pearson to Galton, 28 January 1902 (Galton Papers, 293E).

'purity' of a variety was in itself a theoretical Mendelian concept, not a simple empirical description (Bateson, 1902, 129). Further, Bateson argued that, even if pure-bred peas were used, anomalous results could be produced by such contingencies as accidental crossing, 'sporting' and environmental factors.

The dispute between the biometricians and the Mendelians could not, to use Kuhn's phrase, 'be unequivocally settled by logic and experiment alone' (1970, 94). There was nothing illogical in arguing, as Pearson did, that the best approach to heredity was that which best described the regularities of phenotypic resemblance, nor in placing a priori confidence in a theoretical model and being unabashed at its inability initially to explain anything other than a small range of observed phenomena, as the Mendelians did. Nor could experimental studies of heredity have resolved the issue, even if the two sides had been able to agree on the interpretation of a given result. An undisputable demonstration of an F_2 generation exhibiting a predicted Mendelian ratio would not have converted Pearson and Weldon to Mendelism: they could simply have pointed to the vast range of phenomena not adequately described by Mendelism. Nor, a fortiori, would the failure of Mendelism in a particular case have caused the Mendelians to jettison their basic model. In historical actuality, attempts at 'crucial experiments' did not in any case reach any definite conclusions, but largely degenerated into disputes about the competence/...

competence and honesty of the experimenters (Provine, 1971, 73-80 and 87-8).

The incommensurability of the two positions did lead to difficulties of understanding and communication.⁽⁸⁾ This is particularly the case with the different interpretations of Galton's 'law of ancestral heredity'. As Froggatt and Nevin (1971a; 1971b) emphasise, disputes over the validity of this 'law' were prominent in the controversy. Galton had primarily intended the law, first pointed to in his 1865 paper on 'Hereditary Talent and Character' (1865, 326), to summarise the degree of influence of ancestors of each degree on the height, say, of an individual:

... the influence, pure and simple, of the mid-parent may be taken as $\frac{1}{2}$, of the mid-grandparent $\frac{1}{4}$, of the mid-great-grandparent $\frac{1}{8}$, and so on. (1885c, 261)

Pearson certainly interpreted the law as one of phenotypic resemblance, and attempted to recast it in terms of the theory of multiple regression: as a linear equation giving the predicted height of an individual (in terms of the deviation from the mean height of that individual's generation) as a function of the heights of that individual's ancestors (in terms of the deviation of their heights from the means of their generations) (Pearson, 1898; Pearson, 1903a).

At/...

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(8) 'Mr Bateson and I do not use the same language', wrote Karl Pearson (1902a, 331).

At first sight, Mendelism contradicted this whole approach. Once the genetic characteristics of the parents were known, knowledge of distant ancestry was redundant in predicting offspring characteristics. Thus Weldon could write (1902, 252):

The fundamental mistake which vitiates all work based upon Mendel's method is the neglect of ancestry ... not only the parents themselves, but their race, that is their ancestry, must be taken into account before the result of pairing them can be predicted.

Bateson, in replying, appeared to agree that a fundamental divergence existed between Mendelism and the 'ancestrian' approach (1902, 114):

... the Mendelian principle of heredity asserts a proposition absolutely at variance with all the laws of ancestral heredity, however formulated.

The two sides were, however, talking about different things. The Mendelians had in mind, not phenotypic resemblance, but genetic structure. It was true that on a Mendelian view, distant ancestry was irrelevant, in the sense that what mattered was the composition of the zygote: all individuals with the same zygote were genetically identical, irrespective of where the particular factors had come from. When, however, it came to predicting on a statistical and phenotypic basis the characteristics of offspring, then even on a Mendelian view the characteristics of an individual's ancestry were relevant, as these helped predict the (unknown) parental genetic make-up. As Pearson (1904a; 1909a; 1909b) was able to demonstrate, a multi-factorial Mendelian model in fact led, at the phenotypic level, to a multiple regression/...

gression equation similar to the law of ancestral heredity.

This last development illustrates that difficulties of understanding and communication, while they did exist, were surmountable. In spite of their incommensurability - or, perhaps, because of it, because of the fact that the two approaches were on different ontological levels - there was no absolute formal barrier to a synthesis of the two approaches. Sporadic attempts at reconciliation were indeed made from early on (for example, Yule, 1902). Thus, the analysis of the dispute cannot stop at the demonstration of the incommensurability. We must go on to ask what factors generated and maintained the controversy, and treat the observed incommensurability, in so far as it was persistent, as part of the controversy we are trying to understand.

6.2 Professional Competences

Bateson appears to have felt that the biometricians did not possess (or, in the case of Weldon, were not using) the competences of trained biologists. Thus, he lamented the fact that Galton and Pearson 'were not trained in the profession of the naturalist' (1902, xii). The connection between theoretical Mendelian factors and the observed properties of organisms was not such that anyone could immediately 'see' what was going on. A naive approach, which failed to take account of the complexities of the relationship/...

relationship between theory and the results of particular experiments, could mislead, as Bateson felt had happened in the case of Weldon. Even classification of peas into categories - green or yellow, smooth or wrinkled - could not be done mechanically, as Bateson felt the biometricians did it, but was a difficult task requiring experience (see Bateson to Yule, 28 November 1922; Yule Papers, box 22). The statistical approach of the biometricians was quite inadequate, Bateson told the 1904 meeting of the British Association, in dealing with subtleties of, for example, the creation of new stocks in practical breeding:

Operating among such phenomena the gross statistical method is a misleading instrument; and, applied to these intricate discriminations, the imposing Correlation Table into which the biometrical Procrustes fits his arrays of unanalysed data is still no substitute for the common sieve of a trained judgment. For nothing but minute analysis of the facts by an observer thoroughly conversant with the particular plant or animal, its habits and properties, checked by the test of crucial experiment, can disentangle the truth. (B. Bateson, ed., 1928, 240)

Conversely the biometricians, particularly Pearson, felt themselves to be practising a more rigorous form of biology, which employed exact definitions and mathematical argument. Bateson and the 'old school' of biologists operated with 'confused and undefined notions', the biometricians with 'clear and quantitatively definite ideas' (Pearson, 1902a, 321). The lack of mathematical training of the majority of biologists was blamed by Pearson for what he saw as their indifferent or hostile response to biometry. In the theory of evolution, and some other fields/...

fields of biology, 'without mathematics, further progress has become impossible'.

... mathematical knowledge will soon be as much a part of the biologist's equipment as to-day of the physicist's.
(Pearson, 1902a, 344)

Thus, the participants themselves viewed the controversy as, at least in part, a clash of traditional biological and mathematical competences. How far is it possible to build this insight into an acceptable account of the controversy? One possible approach would be to start with the training individuals receive and their early disciplinary experiences, and to regard these as having a conditioning effect on their future scientific work. This approach is, in effect, that employed by de Marrais (1974). He argues that the mathematical perspective of Galton and the biometricians, in particular their continual use of the normal curve, constrained their perception. It was impossible logically to move from continuous variation to determine a finite number of underlying factors.

... by its very nature the Frequency Law prohibits the discovery of the real (i.e., finite number of) causal agencies determining a trait's distribution pattern or 'type'.
(de Marrais, 1974, 154)

By comparison, Bateson, who was a notoriously weak mathematician, was not constrained in this way.

The nonmathematical basis of William Bateson's (and all the early Mendelians') thought represented not so much a cause of his Mendelism as an absence of the mainstay holding together the bundle of inhibitory relations that held back the biometricians.
(de Marrais, 1974, 169)

However/...

However, the model of the operation of training difference and early experiences implied in arguments such as this seems implausible. To use Wrong's phrase, it would seem to involve an 'oversocialised conception of man' (Wrong, 1976). Without supporting theory or evidence, it is difficult to imagine why individuals should be trapped in this manner by their disciplinary socialisation. After all, there are plenty of instances of individuals breaking with the approach of their training: thus both Weldon and Bateson broke, in different ways, from the morphological and embryological approach to biology of their Cambridge training (Pearson, 1906; Coleman, 1970). Training is obviously of importance, but an individual is not necessarily programmed for life by his or her training.

The internal social structure of science is, as Hagstrom (1965) argues, competitive. Prestige and reward follow from the recognition, by his or her fellows, of the scientist's work as correct and interesting. In this 'market', the scientist's resources are his or her ability to perform successful scientific work, his or her competences. No-one is all-competent. Individuals' competences are competences to use particular techniques, to work within the framework of particular theories, to handle particular materials. Thus, we can expect there to arise a tendency to evaluate new theoretical developments, new techniques, and so on, in terms of their effects on the value of scientists' existing competences. Other things being/...

being equal, we would expect scientists to be favourably inclined to developments which enhance the value of their competences, and hostile to those which devalue them.

Training, on this view, provides individuals with competences, and these competences can affect a scientist's evaluations because of their role as resources in a competitive market for scientific knowledge.⁽⁹⁾

On this view, it is certainly possible to understand the hostility shown by traditional biologists to biometry. If the biometric approach came to dominate biology, as Pearson clearly and publicly hoped, then traditional biological skills would be devalued. E. Ray Lankester wrote (1896, 366):

You can not (it seems to me) reduce natural history, as Prof. Weldon proposes, to an unimaginative statistical form, without either ignoring or abandoning its most interesting problems, and at the same time refusing to employ the universal method by which mankind has gained new knowledge of the phenomena of nature - that, namely, of imaginative hypothesis and consequent experiment.

One of Bateson's favourite bits of advice to young biologists was to 'treasure your exceptions' (B. Bateson, ed., 1928, 324). But there was little room in the biometric approach for the skilled attention of the biologist to the individual case. Biometry would substitute the skills of the mathematician for those of the biologist, and Bateson (along with many/...

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(9) MacKenzie and Barnes (1975, 176-8). This view of the relationship of socialisation and future behaviour is largely taken from Becker (1960; 1964).

many of his colleagues) was no mathematician. Bateson publicly admitted that 'his [Pearson's] treatment is in algebraical form and beyond me' (1902, 110 fn.).⁽¹⁰⁾

Conversely, this view helps us to understand the widespread acceptance of Mendelism by the new generation of professional biologists following the rapid development, by T.H. Morgan and others, of the Mendelian chromosome theory in the period 1910-15 (G.E. Allen, 1975a, 56-65). This new generation had, according to Allen (1975a, 33-9), been trained in an experimental and mechanistic approach to biology (the Entwicklungsmechanik of Roux played a crucial role in this). Initially these biologists (including Morgan himself) were sceptical of the Mendelian approach, which they found too speculative (Allen, 1975a, 53). The establishment of the Mendelian chromosome theory, by the use of the fast-breeding Drosophila melanogaster and the development of techniques such as chromosome mapping, changed their attitude completely. The techniques of Morgan's 'fly room' brought the study of heredity into the sphere of experimental biology. Mendelism then became the key to extending the scope of experimental biology: it was a theory which enhanced the value of the competences of experimental biologists, by showing that the use of these competences could throw new light on traditional biological areas (Allen, 1968, 138/...

(10) Bateson's position should not be overdrawn. He was quite prepared to use elementary statistical techniques in his own work. What he objected to was the subordination of the biological to the mathematical that he perceived in biometry.

138).

This approach to training, then, seems helpful in understanding certain aspects of the controversy: the antagonism of Bateson and traditional biologists to the biometric approach to the study of heredity and biology generally; and the widespread and almost total acceptance of Mendelism by professional biologists in the period 1910-20. However, certain features remain puzzling. One of these, discussed by Coleman (1970), is Bateson's hostility to the very chromosome theory that was responsible for the widespread acceptance of Mendelism.⁽¹¹⁾ Another, more central to the controversy, is Pearson's rejection of the Mendelian approach. For this, surely, cannot be explained in terms of competences. Although Pearson doubted the truth of Mendelism, he was able to operate mathematically within the Mendelian framework with ease, showing a grasp of the consequences of Mendelian heredity in a randomly breeding population far superior to that of any of the British Mendelians.⁽¹²⁾ Mendelism was already a mathematical theory, even if the mathematics involved were simple and crude; a perfect opportunity was opened up for a mathematician to display his competence and the relevance of mathematics for biology by further developing these mathematical/...

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(11) It should be noted, however, that Bateson, who was in many ways not an experimental biologist in the new sense, would not have had shared the experimentalists' reasons for finding the chromosome theory attractive.

(12) See Pearson (1904a; 1909a; 1909b).

mathematical aspects. Thus a 'professional competence' explanation is at best one-sided: it does not appear that it can be used to explain the hostility of the biometricians, as a group of scientists with mathematical competences, to Mendelism.

6.3 Heredity and Evolution

Disagreement between Pearson and Weldon, on the one hand, and Bateson, on the other, was not limited to the question of the validity of Mendelism as a theory of evolution. Even before 1900, the two sides were already in dispute. This dispute had various manifestations: Weldon's (1894) review of Bateson (1894); the controversy on the origin of the cultivated Cineraria; the attacks by Bateson on Weldon's work on crabs and Pearson's work on 'homotyposis'. A common thread ran through all these particular disagreements (for which see, for example, Provine, 1971 and Norton, 1973). That thread was the question of the nature of evolutionary change. The majority of writers on the controversy agree that these early disagreements are crucial to an understanding of the later disagreement over Mendelism (see, especially, Provine, 1971).

The biometricians (Pearson, Weldon and their co-workers, but not Francis Galton) believed that evolution was a process of gradual change, taking place by the selection of continuous variations. If height conferred a/...

a selective advantage, then the mean height of a population would rise gradually from one generation to the next, because each successive generation would be formed by proportionately more offspring of tall parents than of short parents. In this, the biometricians were following Darwin (1859). The orthodox view had never gone unchallenged, even within the community of evolutionists: both T.H. Huxley and Francis Galton had doubted that evolution worked in this way, and had suggested a greater role for discontinuous variations ('sports' or 'saltations'), which differed markedly from the parental generation (Provine, 1971, 10-24). Thus, Galton had felt that evolution might not proceed smoothly, but might 'jerk' from one position of 'stability' to another (1869, especially 367-70 and 375-6). Those opposed to Darwinism also took up the issue of discontinuous variations, although, unlike Huxley and Galton, they tended to suggest that a 'nonmaterial directive agency' was guiding the production of these variations (Provine, 1971, 24).

This long-standing thread of opposition to orthodox Darwinian selectionism was given new force in 1894, with the publication of William Bateson's Materials for the Study of Variation. As the name indicates, the book is mainly a catalogue of a large number of instances of variation. The long introduction, however, conveyed the import of these examples. Bateson argued that the morphological approach to evolutionary theory (in which he had been trained), had proven to be barren: attention had to shift to the empirical study/...

study of variation. This empirical study revealed clearly that large, discontinuous variations did occur in nature. Further, he concluded that it was this type of variation (and not quantitative individual differences) which was of evolutionary significance. Species were discontinuous entities, differing qualitatively from each other: environments, by comparison, shaded continuously one into each other. The source of specific discontinuity could not, therefore, be the environment (whether acting in a direct Lamarckian or indirect selectionist fashion): it had to lie in variation, in the 'raw material' for evolution. Although Bateson said, cautiously, that 'inquiry into the causes of variation is as yet, in my judgment, premature' (1894, 78), he did suggest that the source of discontinuity should be sought 'in the living thing itself', and that the key to its understanding lay in the phenomena of pattern: symmetry and merism (Coleman, 1970, 250).

In the following decade, Hugo de Vries published his Die Mutationstheorie (1901-3), which was in part stimulated by Bateson's work (Allen, 1969, 65). Like Bateson, de Vries thought that large discontinuities were the key to the evolutionary process:

The object of the present book is to show that species arise by saltation and that the individual saltations are occurrences which can be observed like any other physiological process.
(Quoted by Allen, 1969, 59-60)

While Bateson's work had had an impact amongst only those biologists with clear evolutionary concerns, that of de/... favourable assessment of Mendelism, see Darden (1977).

de Vries received wide and generally favourable attention (Allen, 1969, 65-9).

Thus, overlapping the controversy over the validity of Mendelism was this further dispute over the nature of evolutionary change. There was no necessary logical connection between the two issues. For example, Morgan's work from 1910 onwards on mutant Drosophila convinced him mutations could have small phenotypic effects (no greater than the usual limits of continuous variability): he simultaneously upheld Mendelism, a Mendelian mutation theory, and a view of evolution as a gradual process (Allen, 1968). Conversely, de Vries, although one of the three 'rediscoverers' of Mendelism, denied that progressive mutations obeyed Mendelian laws (Allen, 1969, 61), and became disenchanted with Mendelism (Provine, 1971, 68). But, for all this absence of a necessary connection, the two issues became closely bound together, especially in Britain.

In one sense, Bateson came to Mendelism as a result of his belief in the role of discontinuities in evolution.⁽¹³⁾ In the years following the publication of the Materials, he set himself the task of discovering how discontinuous variations might be passed on to successive generations (a key issue in the development of a 'saltationist' theory of evolution). The method he chose was experimental plant hybridisation, the/...

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(13) For an interesting account of the reasons for Bateson's favourable assessment of Mendelism, see Darden (1977).

the crossing of closely related varieties and the examination of the characteristics of sets of offspring of such crosses (Coleman, 1970, 250-1). Bateson was travelling by train from Cambridge to London, to deliver a lecture on the preliminary results of his investigations, when he first read Mendel's paper on peas; he immediately incorporated the results into his lecture (B. Bateson, ed., 1928, 73). He had been 'made ready' for reading Mendel by his own work on discontinuous variations and their heredity. He reacted enthusiastically, and interpreted Mendelism as supporting his own 'saltationist' evolutionary views. He wrote (B. Bateson, ed., 1928, 223):

The discovery of Mendelian elements admirably coincided with and at once gave a rationale of these facts.

Pearson and Weldon also felt there to be a connection between Mendelism and a discontinuous view of evolution: but this, for them, was a reason to reject Mendelism, not to embrace it. Pearson wrote (1906, 306):

To those who accept the biometric standpoint, that in the main evolution has not taken place by leaps, but by continuous selection of the favourable variation from the distribution of the offspring round the ancestrally fixed type, each selection modifying pro rata that type, there must be a manifest want in Mendelian theories of inheritance. Reproduction from this standpoint can only shake the kaleidoscope of existing alternatives; it can bring nothing new into the field. To complete a Mendelian theory we must apparently associate it for the purposes of evolution with some hypothesis of 'mutations'.

Thus, the biometricians' opposition to Mendelism can be seen, at least in part, as an opposition to the saltationism with which/...

which they associated it.

So the problem of explaining the biometrician/Mendelian controversy is one of explaining these divergent views of evolution, at least in so far as we wish to explain the prior dispute between the biometricians and Bateson, and its continuance into the later phase of the controversy over Mendelism proper. These divergent views on evolution cannot be seen as arising from experimental evidence, as they took the form of assumptions, rather than conclusions. Bateson (1901) separated variation by definition into the two classes of 'specific' variations (which were discontinuous and of evolutionary significance) and 'normal' or 'continuous' variations (which a priori were not), and criticised Pearson et al. (1901b) on the grounds that Pearson had not done so. And, as the quotation above indicated, Pearson took the continuous view of evolution as 'the biometric standpoint', i.e. as a presupposition.

Factors internal to the social system of science, such as professional competences, may again be examined as a possible grounding for these different views of evolution. Allen (1969) shows how de Vries's mutation theory appeared initially to solve some of the problems which troubled the Darwinian theory (see also Darden, 1976). Biologists might then be expected to take up the new theory as a promising area for innovative work. In particular, the mutation theory gave new relevance to experimental work in the form of/...

of attempts to demonstrate mutations in plants and animals reared in experimental conditions. Mayr (1973, 149) states of the period immediately after 1900:

I am not aware of a single experimental... biologist who championed natural selection.

Old-fashioned field naturalists, by comparison, tended, according to Mayr, to remain faithful to orthodox Darwinism.

It would therefore seem plausible to suggest that the assessment of evolutionary theories by experimental biologists was informed by judgments of the relative scope offered by these theories for experimental work, and that it was in part for this reason that they preferred the mutation theory to orthodox Darwinism. An instance of this would appear to be C.B. Davenport. Davenport had introduced Pearsonian biometry to America, but following the 'rediscovery' of Mendelism and the publication of the Mutationstheorie he 'defected'.⁽¹⁴⁾ Davenport had considerable experience as an experimentalist, and had introduced the teaching of experimental morphology to Harvard. A 'painfully ambitious' man, he was from 1902 to 1904 engaged in a campaign to persuade the Carnegie Institution to set up a station for the experimental study of evolution. He therefore approached the mutation theory with a strong interest in the experimental studies it made possible. In 1902, he toured Europe, visiting the Marine Biological Stations/...

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(14) For biographical details of Davenport, see MacDowell (1946) and Rosenberg (1961).

Stations there 'to better fit myself for the work of directing the Station for Experiments on Evolution, whenever the Carnegie Institution establishes it' (quoted by MacDowell, 1946, 19). On his return Davenport wrote (1903, 46):

The most important events relating to the study of variation that have occurred during the past two years have been the establishment of the journal Biometrika, the foundation in America of a Society of Plant and Animal Breeding, the completion of the first volume of de Vries's 'Mutations-theorie', and the rediscovery of Mendel's Law of Hybridity. Especially the latter two events have awakened a strong tendency toward the experimental study of evolution.

During the last four months the recorder has visited many of the experimental evolutionists of Europe. While the total work on this subject in Europe is of the greatest importance, it is carried on under conditions that greatly hamper the work and make it impossible to start experiments that require to be carried on for a long period of years. Everywhere the hope was expressed that in America a permanent station for experimental evolution would be founded, and it was believed that the Carnegie Institution would be the proper organisation to initiate and maintain such a station.

Thus, we can claim that for Davenport the mutation theory and Mendelism made it possible to do more (within his desired occupational role) than the Darwinism of the biometricians. In 1904 he did indeed achieve his aim of becoming Director of a Laboratory set up by the Carnegie Institution at Cold Spring Harbor, and the work done under his direction was Mendelian and mutationist in tendency. As he wrote, reviewing the work of de Vries (1905, 369):

The great service of de Vries's work is that, being founded on experimentation, it challenges to experimentation as the only judge of its merits. It will attain its highest usefulness only if it creates a widespread stimulus to the experimental investigation of evolution.

Such/...

Such an attitude to mutation was incompatible with collaboration with Pearson, and Pearson and Davenport split violently.⁽¹⁵⁾

Professional competences thus seem to have played a part in the overall dispute over evolution. Once again, however, their role seems to be weaker when we approach the central figures in the controversy in Britain. Bateson, Pearson and Weldon were not primarily experimentalists. In some ways both Bateson and Weldon were closer to the old-style naturalist than to the new professional experimental biologist; Pearson, of course, was by training not even a biologist. Therefore, it would seem desirable to seek an alternative 'grounding' of the radically different views on evolution exemplified in the work of the major participants in the controversy.

6.4 A Structural Hypothesis

In order to go beyond consideration of professional competences and to develop a fuller explanation of the controversy in terms of the sociology of knowledge, it is necessary to step back for a moment from the details of the dispute. As has been argued in sections 1.4 and 4.2, causal explanations of the beliefs of particular individuals cannot/...

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(15) The split can be followed in their correspondence in the Davenport Papers.

cannot be provided by the sociology of knowledge: individuals such as Karl Pearson rather have to be taken as examples, perhaps of a particularly striking nature, of the relationship between knowledge, social interests and the social structure. So I shall start by suggesting a sociological hypothesis connecting the issues of evolution and inheritance debated by the biometricians and Mendelians to social interests. Then, in section 6.5, I shall examine material from the writings of the major participants in the controversy which can be taken as evidence for the hypothesis.

One aspect of the hypothesis arises fairly directly out of the analyses of chapters three and four. Stated baldly, I suggest that the distinctive judgments and technical developments manifested in the biometric approach to evolution and inheritance can be related to the social interests of the rising professional middle class. The obverse of this claim was suggested by the work of William Coleman (1970) on Bateson, although, in using Coleman's analysis in this way, I am going beyond what he himself argues.⁽¹⁶⁾ The account of evolution and inheritance produced by Bateson and his closest followers can, I would claim, be seen as sustained by the interests of those whose social position was threatened by/...

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(16) Coleman's work is best seen as a contribution to the history of ideas, not the sociology of knowledge. He presents what seems to me a persuasive analysis of Bateson's belief system, but does not seek to relate this to the social structure in any fully explicit way.

by economic growth and the social changes consequent upon the industrialisation and modernisation of British society.

It is not suggested that this hypothesis provides a complete explanation of the controversy. As well as not claiming to explain individual behaviour, the hypothesis does not contradict the assertion that social interests related to professional competences can be seen as operating in the controversy. To use, in loose analogy, the terminology of Althusser (1969, 87-116), concrete conjunctures are in general overdetermined: social factors of more than one type have to be considered in explaining any particular episode. Nevertheless, the structural hypothesis put forward here may throw light on aspects of the controversy which otherwise remain puzzling.

Consider biometry. Within biometry, scientific judgments reflected a cognitive interest in predicting and controlling the overall incidence of characteristics within populations. While in asserting this nothing is claimed about the motives of particular individual biometricians, it helps us understand certain key aspects of the biometric school considered as an institution and of their work considered as a 'paradigm' or 'disciplinary matrix' (Kuhn, 1970). Thus, in the work of the biometric school biological populations were taken as the unit of analysis in studies of evolution and heredity: individual instances were of importance only in the aggregate. The definitions and techniques/...

techniques used by the biometricians referred to populations and their characteristics. These definitions and techniques (spelt out most explicitly in Pearson, 1896) were predicated on a descriptive and predictive model of the evolutionary process, in which inheritance and selection operated in a measurable fashion to produce definite effects on succeeding generations.

We, therefore, require a generalised investigation of the following kind: Given $p + 1$ normally correlated organs, p out of these organs are selected in the following manner: each organ is selected normally round a given mean, and the p selected organs, pair and pair, are correlated in any arbitrary manner. What will be the nature of the distribution of the remaining $(p + 1)$ th organ?

... If the p organs are organs of ancestry - as many as we please - and the $(p + 1)$ th organ that of a descendant, we have here the general problem of natural selection modified by inheritance.
(Pearson, 1896, 298; emphasis deleted)

This model made possible the prediction of the effects of intervention, whether of the animal breeder or the eugenicist, in animal or human populations, and allowed, for example, calculations of the duration of selection needed to establish new breeds (Pearson, 1896, 314-8). It was not that the biometricians studied evolution, tested their model, and then decided that evolution was a process that had certain characteristics, such as acting on populations as aggregates of individuals. These characteristics were rather pre-suppositions of the biometric view: they were simply what the biometricians meant by evolution. ⁽¹⁷⁾

Biometry/...

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(17) This comes over most clearly in their criticisms of Bateson (see Weldon, 1894 and Pearson, 1902a).

Biometry was, as has been discussed in chapters four and five, closely tied to eugenics. No distinction was drawn by the biometricians between human populations on the one hand, and plant and animal populations on the other, as far as the techniques for the analysis of evolutionary change were concerned. Indeed, the central tenet of all social Darwinism was the parallelism between the two spheres. The application of biometric techniques to man thus involved a model of human society which had certain characteristic features. Human populations were atomistic and aggregative in their properties, and change was a process that took place on a population level. On this model, social change could indeed be seen as evolution. From generation to generation, the statistical characteristics of the individuals making up in aggregate a given population altered slowly in response to the effects of natural selection, assortative mating and differential fertility.

Hence, a relationship existed between the cognitive interest underlying biometry - in the prediction and control of the overall incidence of characteristics within populations - and the interests of the rising professional middle class. As analysed above, this social group can be seen as having an interest in interventionist, scientific measures of reform and social control, such as were exemplified in Fabianism and eugenics. These would both increase the occupational positions open to its members and increase its status as the possessor of the specialised knowledge necessary for/...

for such schemes. The atomistic, aggregative, populational model of society was a model in which the potential for planned social intervention was at a maximum. If society was the sum of its individual parts, and classes and communities simply names for collections of individuals and not entities sui generis, then the effects of any given course of action were easily predictable. One had, as it were, simply to measure the effects on each individual, and add them up for the population. Take eugenic intervention as an example. The eugenicists assumed that the 'unfit' could be identified as individuals, and incarcerated or sterilised, with predictable benefits in the next generation. This scheme would, of course, have run into great difficulties if it had encountered opposition on a class or community basis, if it had been taken as directed, not against individuals, but against collective entities. Its success was thus, to a certain extent, predicated on the individualistic, atomistic model of society: indeed, its decline in the inter-war period may in part be attributed to the declining creditability of that model, as instanced by the difficulty of treating unemployment on the mass scale of the 1920's and 1930's as the result of individual inadequacy.

In assuming this model of society, Pearson and the eugenicists did not consciously choose between alternatives: it is implicit, rather than explicit, in their thought. It was certainly not a hypothesis to be tested, but rather was the basis of their procedures and proposals. While/...

While it was arguably the dominant image of society in Victorian Britain, it was by no means the only one. Other models would have been available to them, had they needed them. Their interests, however, led them to select, albeit quite unconsciously, a model of society which allowed the greatest scope for the types of policy they wished to pursue. (18)

What was the detailed connection between this model of society and the biometric view of evolution? It is not my argument that the biometricians as a group of individuals came to hold a view of society and then selected a view of nature congruent with it. Such an argument could perhaps be made for Karl Pearson, but it misses the essential point. In Victorian and Edwardian Britain the connections between evolutionary biology and images of society were institutionalised/...

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(18) This is not in itself to be deprecated: revolutionary socialists and conservatives can be seen as doing the same thing. It would, after all, be strange to see a group selecting a model of society in which the potential for its policies to work was small.

Of course, it could be suggested that the opposite process takes place as well: that, on the basis of a model of society, groups select what seems the most appropriate policy. Undoubtedly this happens; but in answering the consequent question, of why a given model of society should be held, we are again led back to consideration of social interests. For reasons of simplicity, it seems best to hold, at a first approximation, to the above formulation, despite its obvious crudity.

institutionalised.⁽¹⁹⁾ To say something about nature (in the sense of, say, a general pronouncement about evolution) was to say something about society. In an enterprise such as biometry/eugenics, society and nature were inextricably intertwined: the option of saying 'A holds for nature' but 'not-A holds for society' was neither taken nor, apparently, considered.⁽²⁰⁾ Individuals may well have been unconcerned with the implications that biological theorising would be taken, through these institutionalised connections, as having: Pearson's colleague Weldon may have been an instance of this. These implications and connections were nonetheless real. If Weldon had succeeded in his attempt to demonstrate the empirical validity of orthodox Darwinism (Weldon, 1895), social Darwinism and eugenics would thereby have been strengthened.

Against the biometric view of evolution was ranged a different view. It emphasised the role of individual instances of variation rather than population processes; the discontinuous and relatively unpredictable nature of change as against its gradual and law-governed nature; and the holistic integrity of the organism rather than its plasticity/...

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(19) See, for example, Young (1969).

(20) It is interesting to note in passing Helfand's recent re-evaluation of Huxley's Evolution and Ethics (1893), which has conventionally been seen as an instance of the taking of this option. Helfand (1977) argues that Huxley was attacking, not social Darwinism in general, but only those forms of it to which he was politically opposed: he was pursuing an argument within a broadly social-Darwinian framework.

plasticity.⁽²¹⁾ Again, a parallel view of society can be found. This highlighted the role of the individual genius rather than the characteristics of the mass; it foresaw not gradual progress but sudden catastrophe; it was pessimistic about the possibility of reform; and it deployed the image of society as an organism to attack the atomistic individualism of bourgeois society.

Clearly Coleman (1970) has some justification in referring to this as a 'conservative style of thought'.⁽²²⁾ In Mannheim's original analysis (Mannheim, 1953), German conservatism was seen as sustained by the social interests of a landed aristocracy fundamentally opposed to bourgeois revolution, and to the 'natural law' style of thought that formed its typical ideological counterpart. The British aristocracy was, however, not generally in opposition to the bourgeoisie but in a situation of accommodation with it (Barrington Moore, 1967, 3-39). No single major social locus/...

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(21) This view was that held by Bateson and, perhaps to a lesser extent, Punnett, but, as Coleman (1970) points out, certain connections with the earlier views of opponents of Darwinism such as Butler and Mivart can be seen.

(22) Some of the chief characteristics of conservative thought are, according to Mannheim (1953), its opposition to rationalist individualism (of which utilitarianism would be the best British example), its elevation of 'being' over 'thinking', of the whole over the parts, of the particular over the general, and the traditional over the progressive. Conservatism took issue with the mechanism and atomism of bourgeois thought, opposing to it holism and the metaphor of the social organism, and emphasizing the qualitative rather than the quantitative. Mannheim is clear that these are general characteristics, and not all to be found in the thought of any one conservative thinker.

locus of conservatism, in Mannheim's sense, appears to have existed in Victorian Britain. Nonetheless, conservative hostility to bourgeois 'progress' could be expected: not all sections of old elites were able to come to an amicable peace with, or share in the profits of, industrial capitalism. Such conservative romanticism does indeed form a persistent, if diffuse, strain in British intellectual life in the Victorian and Edwardian periods.⁽²³⁾

6.5 Some evidence

A full discussion of evidence for and against the hypothesis put forward in the previous section would go far beyond the scope of this thesis, involving as it would wide-ranging studies of the social uses of evolutionary beliefs, of the relations of these uses to the social structure and to social interests, and of the variations of these uses through time. The discussion here will be limited to the writings of the two major figures discussed in this chapter, Pearson and Bateson; Bateson's social background and its relations to his thought will also be examined.

The material presented in this section (and in section 6.6, which extends this discussion to R.A. Fisher's 'resolution' of the controversy) should be taken only as relevant evidence. The hypothesis put forward in section 6.4/...

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(23) See, in particular, Williams (1968), but also Perkin (1972, 237-52 and 262-4) and Levitas (1976).

6.4 suggests simply the existence of connections between evolutionary beliefs and social interests. It does not suggest that all persons of a given social background would necessarily adopt one position rather than another. As the case of Bateson, discussed below, shows, individual biographies are complex, and in any case family background is only one amongst several social factors of relevance. Nor does the hypothesis assert that a given position can be adopted for only one reason. Thus, as discussed in sections 5.2 and 5.3, individual motives for adherence to the biometric school were possibly very varied. The position, for example, of Pearson's major co-worker W.F.R. Weldon is unclear in the matter of the socio-political connotations of biometry. He never actively supported eugenics and Pearsonian social Darwinism; nor did he ever make any apparent attempt to distance himself from them.⁽²⁴⁾ So, in suggesting the existence of connections between biological beliefs and social interests, it is by no means implied that these connections were apparent as motives in all individual cases: particular scientists may well have chosen to ignore them, or have been uninterested in them. The evidence for the hypothesis presented here is simply that of the manifestation, in the writings of Pearson and Bateson, of these connections. Clearly this evidence in no/...

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(24) Weldon's letters to Pearson (Pearson Papers) do give some suggestions of social attitudes broadly congruent with Pearson's; for example, support for imperialism, hostility to upper-class superficiality, approval of the 'middle classes'.

no way proves the validity of the hypothesis: it may, however, go part of the way to indicating its plausibility.

In the case of Pearson, part of the evidence concerns the parallelism of his early views on social change (section 4.4) and his later views on biological change, in particular the emphasis in both on the continuity of change and its consequent predictability. This is discussed in MacKenzie and Barnes (1975), although the emphasis on it in that paper may be misleading. Further evidence is provided by some of his detailed reasons for opposing Mendelism, especially as applied to the key nexus between biology and society, eugenics. Mendelism in general he may have disliked: its eugenic application he found dangerous. He and two collaborators wrote (Pearson, Nettleship and Usher, 1913, 491; quoted by E.S. Pearson, 1936-8, part 2, 169-70):

The problem of whether philosophical Darwinism is to disappear before a theory which provides nothing but a shuffling of old unit characters varied by the appearance of an unexplained 'fit of mutation' is not the only point at issue in breeding experiments. There is a still graver matter that we face, when we adduce evidence that all characters do not follow Mendelian rules. Mendelism is being applied wholly prematurely to anthropological and social problems in order to deduce rules as to disease and pathological states which have serious social bearing. Thus we are told that mental defect, - a wide term which covers more grades even than human albinism, - is a 'unit character' and obeys Mendelian rules; and again on the basis of Mendelian theory it is asserted that both normal and abnormal members of insane stocks may without risk to future offspring marry members of healthy stocks. Surely, if science is to be a real help to man in assisting him in a conscious evolution, we must at least avoid spanning the crevasses in our knowledge by such snow-bridges of theory. A careful record of facts will last for ages, but theory is ever in the making or the un-making/...

making, a mere fashion which describes more or less effectually our experience. To extrapolate from theory beyond experience in nine cases out of ten leads to failure, even to disaster when it touches social problems. In all that relates to the evolution of man and to the problems of race betterment, it is wiser to admit our present limitations than to force our data into Mendelian theory and on the basis of such rules propound sweeping racial theories and inculcate definite rules for social conduct.

Pearson's evolutionism was designed to provide a means for controlling social change. This did not simply constrain the content of evolutionary biology by ruling out, as the first sentence of the above quotation indicates, theories such as Mendelian mutationism which formulated evolution as a discontinuous and unpredictable process. It also constrained its form. To be credible, social programmes such as eugenics had to be seen as based on sure knowledge, not knowledge that was subject to future retraction and contradiction. For Pearson, this meant that a theory of evolution and heredity had to be developed from observational data, and from these alone. Knowledge of the 'facts' was stable and a safe basis for social action. Theory which went beyond the facts was, however, subject to 'fashion', to change. Thus, evolutionary biology should be phenomenalist, not theoretical, in its form.

Pearson felt that his own approach met this criterion. As outlined above, his notion of heredity was a phenotypic, phenomenalist one. Biometry attempted to 'display' evolution as measurable mass change in population distributions. The mathematical apparatus presented in Pearson/...

Pearson (1896) took observational data and analysed it according to multiple regression models. The law of ancestral heredity, according to Pearson, was derived from observational data, and enabled the apparently theory-free prediction of offspring characteristics from ancestral characteristics. The effects of eugenic intervention were predictable, without any biological theory of heredity, because Pearson's concept of heredity simply summarised what happened in the 'passage' of a characteristic from given individuals in one generation to those in the next. Theory-free control, as well as theory-free prediction, was thus apparently possible.

Early Mendelism, by comparison, was obviously theoretical. A simple exemplar was being imaginatively and sometimes rashly deployed, and was being modified in what often seemed an ad hoc fashion. Pearson wrote (1906, 306):

The simplicity of Mendel's Mendelism has been gradually replaced by a complexity as great as that of any description hitherto suggested of hereditary relationships ... The old categories are, as Weldon indicated, being found insufficient, narrower classifications are being taken, and irregular dominance, imperfect recessiveness, the correlation of attributes, the latency of ancestral characters, and more complex determinantal theories are becoming the order of the day.

With hindsight, we can identify this as creative science, as simply the growth of genetic knowledge. But for Pearson, who sought in the study of heredity the basis of an applied social science of evolution, this process could have scandalous consequences.

The/...

The most serious of these was when Davenport (1910) suggested that feeble-mindedness was a simple Mendelian recessive, and went on (Davenport, 1911) to argue that a whole range of characteristics of eugenic importance were of a similar nature. Davenport drew from this what seemed to Pearson to be not merely a foolish, but an immoral conclusion:

Weakness in any characteristic must be mated with strength in that characteristic; and strength may be mated with weakness. (1910, 25)

A devastating criticism of Davenport's work eventually appeared from Pearson's department, showing how Davenport's methods were biased towards producing the simple Mendelian results he sought. It concluded (Heron, 1913, 62):

The future of the race depends on the strong mating with the strong, and the weak refraining from every form of parenthood. Nothing short of this rule will satisfy the true Eugenist.

In the course of time, Mendelians themselves came to reject Davenport's simplistic analyses. Bateson was always doubtful (B. Bateson, ed., 1928, 341 fn.), although in the 1920's Punnett still assumed that feeble-mindedness was a simple recessive trait (Punnett, 1925, 705). Pearson's point, however, was that unjustified theoretical extrapolations, even if subsequently retracted, could have disastrous anti-eugenic consequences. Eugenics could not be based on a fallible theory: it had to be based on 'hard fact', reliable prediction, and thus unerring control.

Norton (1975a; 1975b) has drawn particular attention to Pearson's phenomenalism as a cause of his opposition to Mendelism/...

Mendelism, and points to the fact that this phenomenalism was advanced prior to the rediscovery of Mendelism in Pearson (1892a). Norton's view, it seems to me, has to be supplemented in two crucial aspects. Firstly, Pearson's positivist and phenomenalist philosophy of science reflects the general position of science in his thought, as the arbiter of morality and social change (see section 4.7). Speculative, imaginative theorising had to be eliminated from a science intended for this role. Thus, it is possible to see Pearson's general phenomenalism as reflecting his view of the social role of science: his particular evolutionary phenomenalism reflected the social role of evolutionary science. Secondly, it would be mistaken to see Pearson's science as determined in detail by his overall philosophical views. Some of his science indeed looks very strange when judged in these terms (an example is discussed in the next chapter). His overall philosophical aims were thus at best only a partial determinant of his science. More particular factors, such as the connection between scientific theorising and eugenics, have also to be taken into account. In the case of Mendelism, this connection 'ran parallel' to his overall philosophy: in the episode discussed in the next chapter it partly cross-cut it, and proved in that instance stronger than it.⁽²⁵⁾

Bateson was, by comparison with Pearson, a much less 'public' thinker. In the case of Pearson, there is clear documentary evidence of social and political views preceding/...

- (25) Norton suggests another possible cause of Pearson's opposition to Mendelism: his upholding of a Weltbild in which it was denied that any two objects were totally alike (as Mendelian factors in a sense were). It seems to me that, to the extent that Pearson held to this, it can best be seen as a generalisation from his biological beliefs and experience, rather than as a determinant of them. That is how I would interpret the following, which is perhaps his most explicit statement on the matter (Pearson, 1914-30, 3A, 84 fn.):

I must confess to feeling it extremely difficult to accept the view that the population of germ cells belonging to an individual organism are like atoms, identical in character, and have a germinal capacity defined by absolutely the same formula. Such a population of germ cells is, if parasitical, still an organic population, and one continually in a state of reproduction and change. No other organic population that we know of is without variation among its members ...

This interpretation is, I think, supported by Norton's account (Norton, 1975a) of the origins of Pearson's belief in biological variability,

That William Bateson was justly described as his social and political thought as, in Marshall's words, 'conservative' seems clear. His general essays (reprinted in M. Bateson, ed., 1928) reveal a man deeply opposed to militarism and to what he called 'militant individualism', who played his faith in exceptional genius, especially of an artistic kind, who hated the vulgar commercial imperialism which he felt to lie behind the Boer War and First World War, who hankered after the static social order of feudalism to replace an industrial capitalism which he felt to be socially unnatural and ecologically doomed. (26) Bateson's conservatism...

(26) See, for example, M. Bateson (ed.) (1928), 13-16, 128-42, 247, 304, 337, 456-71.

preceding his views on biology. In the case of Bateson, this is not so. Yet Bateson's writings provide sufficient evidence to allow it to be claimed that, in his case too, connections between social interests and biological thought are manifested.

Dr Alan Cock, who is writing a biography of Bateson, believes several of the particular points made by Coleman (1970) regarding Bateson's 'conservative thought' and, in particular, its relationship to Bateson's hostility to the chromosome theory, to be wrong. A full judgment on this issue must obviously be withheld until the publication of Cock's work. On my reading of Bateson, it would seem that Coleman has achieved an insight into his thought, even if some of Coleman's detailed arguments are subsequently disproven.

That William Bateson can justly be described in his social and political thought as, in Mannheim's sense, 'conservative' seems clear. His general essays (reprinted in B. Bateson, ed., 1928) reveal a man deeply opposed to egalitarianism and to what he called 'malignant individualism', who placed his faith in exceptional genius, especially of an artistic kind, who hated the vulgar commercial imperialism which he felt to lie behind the Boer War and First World War, who hankered after the stable social order of feudalism to replace an industrial capitalism which he felt to be socially unnatural and ecologically doomed.⁽²⁶⁾ Bateson's conservatism/...

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(26) See, for example, B. Bateson (ed.) (1928, 15-16, 128-42, 347, 354, 357, 456-7).

conservatism was not of an activist kind. He was completely disillusioned with party politics and almost totally pessimistic about reversing the trend of social change by reformist means. In one area only did he move from thought to action: the defence of his beloved Cambridge from the encroachments of industrial society.⁽²⁷⁾

What may be more contentious than this characterisation of Bateson's social and political thought is Coleman's central claim that Bateson, in his science, can be seen as a conservative thinker. Coleman's argument rests chiefly on the 'style' of Bateson's science, on what he claims to be its emphasis on experiential concreteness and on the aesthetic, on pattern and form and on visual metaphors. Rather than discuss this general characterisation, I will concentrate instead on more specific instances of overt connections of the social and the biological in Bateson's work. Before doing this, it is, however, necessary to discuss one immediate and obvious objection to any characterisation of Bateson's science as, in Mannheim's sense, conservative thought.

On Mannheim's schema, atomism is a general characteristic of natural-law thought, and not of conservative thought, which counterposes holism to atomism. Yet Bateson was a Mendelian, and surely Mendelism is the archetype of reductionist/...

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(27) This is further discussed below.

reductionist atomism? The interesting point about Bateson, at least on Coleman's analysis, is, however, precisely Bateson's hostility to those chromosome theorists who most fully developed the atomic metaphor in Mendelism by, in effect, reducing the gene to a material particle. As against their literal atomism, Bateson developed an alternative metaphor which, while still mechanical, emphasised holistic ordering rather than 'billiard ball' materialism. Animals and plants are not matter, wrote Bateson, they are 'systems through which matter is continually passing'. On this view:

The cell ... is a vortex of chemical and molecular change ... We must press for an answer to the question, How does our vortex spontaneously divide? The study of these vortices is biology, and the place at which we must look for our answer is cell division.

(Quoted by Coleman, 1970, 274-5)

Coleman (1970, 264-9) makes the interesting suggestion that the source of Bateson's alternative metaphor was the ethereal, non-material vortex atom of the Cambridge physicists. The latter have themselves been analysed by Wynne (1977) as exhibiting a conservative style of thought.

Holism played an important part in Bateson's biological thinking. His son Gregory writes of him (G. Bateson, 1973, 349):

In the language of today, we might say that he was groping for those orderly characteristics of living things which illustrate the fact that organisms evolve and develop within cybernetic, organisational and other communicational limitations.

Early letters to his sister Anna, taken together with the Materials/...

Materials, reveal William Bateson's early evolutionary thinking as centring round his dissatisfaction with what he saw as the impoverished view of the organism in orthodox Darwinism and his search for an alternative way of conceptualising the organism as an integrated, patterned whole.⁽²⁸⁾ Orthodox Darwinism he criticised as a 'utilitarian view of the building up of Species' (1894, 11). The manifest lack of utility of many specific characteristics, such as plumage, and the fact that many useful characteristics could only be useful if perfect (and thus could not have arisen gradually), were for him strong arguments against this 'utilitarian' selectionism.

It would be too speculative to place much weight simply on Bateson's choice of the term 'utilitarian' to describe what he opposed in accepted evolutionary theory.⁽²⁹⁾ It is interesting, however, that at precisely the time when Bateson was developing his opposition to orthodox Darwinism he was conducting his major campaign in Cambridge University politics. He was a leader of the opposition to the abolition of the compulsory entrance qualification in classical Greek. It may seem strange that a man who was a scientist and not a classical scholar should choose such an/...

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(28) Bateson (1894), and B. Bateson (ed.) (1928, 39-43).

(29) Coleman does, however, suggest that for Bateson, 'Darwinism hued all too closely to the blighted atomistic individualism of the utilitarians' (Coleman, 1970, 295).

an issue to devote his energies to, but for Bateson compulsory Greek was of enormous symbolic importance. At stake was the 'Classical System' as against mere 'Technical Education'. Mathematics was, he felt, compulsory for the wrong reasons: it was useful 'in trade and professions for the making of money' (quoted by Crowther, 1952, 252). Greek, by comparison, was a means of social control and enculturation:

In the arid mind of many a common man there is an oasis of reverence which would not have been there if he had never read Greek. For Society it would be dangerous, and for the common man it would be hard, if he had never stood thus once in the presence of noble and beautiful things.
(B. Bateson, ed., 1928, 48)

Those who came to Cambridge from 'the Black Country of the commonplace' had to be exposed to the 'side of life which is not common' (B. Bateson, ed., 1928, 48). To remove the entrance qualification in Greek would lead to the selection for Cambridge of those who, in the words of his wife, had 'educational aims ... so utilitarian as to be properly placed outside the University pale' (B. Bateson, ed., 1928, 49).

Bateson's broadsheet on compulsory Greek suggests a conscious connection between his attacks on utilitarianism in education and in biology. He admitted that the Classical System was 'useless'. However,

... from grim analogies in Nature it must be feared that it is in just this 'uselessness' that the unique virtue of the [Classical] System lies.

(B. Bateson, ed., 1928, 48)

It seems possible that there was a link between Bateson's social/...

social defence 'of the things which are beautiful and have no "use"' (B. Bateson, ed., 1928, 48) and his attack on a biological utilitarianism that held that

... living beings are plastic conglomerates of miscellaneous attributes, and that order of form or Symmetry have been impressed upon this medley by Selection alone.
(W. Bateson, 1894, 80)

The link may have been a common concern for the necessary conditions of holistic order and stability, whether social or biological, as against exclusive concern for the 'useful'. The chief social lesson of biology, for Bateson, was of the need (and indeed inevitability) of a return to an essentially feudal social order (B. Bateson, ed., 1928, 354) to replace the competitive commercial individualism of Victorian and Edwardian Britain.

One expression of Bateson's hostility to orthodox Darwinism was thus his development of a holistic view of the organism which emphasised those aspects of it, the phenomena of pattern and symmetry in particular, which could not be seen as 'useful'. The publicly more prominent aspect was, of course, his championing of discontinuity. Here again, the social and the biological intermingled in his writings. He opposed, both socially and biologically, the biometric view of evolution as an orderly, predictable process based on gradual changes in the aggregate. Real advance came, he felt, from rare and largely unpredictable discontinuities, whether the appearance of a 'sport' in biology or an exceptional 'genius' in society. The 'genius' and the 'sport' /...

'sport' were indeed identified:

It is upon mutational novelties, definite favourable variations, that all progress in civilisation and in the control of natural forces must depend.

(B. Bateson, ed., 1928, 353)

... we have come to recognise that evolutionary change proceeds not by fluctuations in the characters of the mass, but by the predominance of sporadic and special strains possessing definite characteristics ...

(B. Bateson, ed., 1928, 354)

Given the crucial role of eugenics in expressing the connection between society and biology in Pearson's thought, Bateson's attitudes to eugenics take on particular interest. Bateson was just as much of a hereditarian as any of the eugenicists, and quite happy to interpret class differences in genetic terms. He showed no compassion for most of those on whom the practice of negative eugenics was proposed. He wrote of the 'feeble-minded':

The union of such social vermin we should no more permit than we would allow parasites to breed on our own bodies.

(B. Bateson, ed., 1928, 306)

Eugenics disquieted him, however. Its reforming nature was alien to his pessimistic conservatism:

The kind of thing I say on such occasions [talks on eugenics] is what no reformer wants to hear, and the Eugenic ravens are croaking for Reform ...

(B. Bateson, ed., 1928, 388)

He disliked what he saw as the narrowly middle class values of the eugenics movement. Why did the eugenicists focus on convicted criminals, and not on the 'army contractors' and 'newspaper patriots', he asked.

Consistent and portentous selfishness, combined with/...

with dulness of imagination are probably just as transmissible as want of self-control, though destitute of the amiable qualities not rarely associated with the genetic composition of persons of unstable mind.
(B. Bateson, ed., 1928, 374)

He would 'shudder', he said, when he read Galton's condemnations of 'Bohemianism'. He suggested that Galton had too much respect for 'material success'.

In the eugenic paradise I hope and believe that there will be room for the man who works by fits and starts, though Galton does say that he is a futile person who can no longer earn his living and ought to be abolished. The pressure of the world on the families of unbusinesslike Bohemians, artists, musicians, authors, discoverers and inventors, is serious enough in all conscience ... Broadcloth, bank balances and the other appurtenances of the bay-tree type of righteousness are not really essentials of the eugenic ideal ... I imagine that by the exercise of continuous eugenic caution the world might have lost Beethoven and Keats, perhaps even Francis Bacon, and that a system might find advocates under which the poet Hayley would be passed and his friends Blake and Cowper rejected.
(B. Bateson, ed., 1928, 374-5, 377)

Bateson, then, was torn. He recognised eugenics as a movement of the 'intellectual and professional class' (B. Bateson, ed., 1928, 387) to which he belonged. Yet its success would have merely continued the process of the encroachment of utilitarian rationalisation and modernisation against which he had set himself.

The case of Bateson illustrates in interesting fashion why a deterministic sociology of knowledge applied at the individual level is impossible. In his occupational position, Bateson belonged to the professional middle class, as/...

as indeed did his father, W.H. Bateson, Master of St. John's College, Cambridge. Both his grandfathers were Liverpool businessmen.⁽³⁰⁾ He himself developed a conservative ideology, and in doing so broke with his family background (his father was a leading university reformer and Liberal). Yet that does not mean that his conservatism was suspended in the air, unconnected to any social formation. The most obvious social formation to which one might seek to connect it was the aristocracy. But, while Bateson clearly held the old aristocracy in high regard, he had no illusions in them as a social force:

The old aristocracy has largely gone under, not because it had not great qualities, but because those qualities were not of a kind that count for much in the modern world.
(B. Bateson, ed., 1928, 417)

In any case, he clearly saw himself as a member of the 'intellectual classes', and not of the aristocracy. What appears instead to have been crucial to Bateson's conservatism was Cambridge University. This social institution formed the background of his early life (his father had already been Vice-Chancellor of Cambridge University at the time of his birth): as suggested above, his political energies were largely/...

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(30) Apparently on this basis, Crowther (1952, especially 256 and 289) suggests that Bateson should be placed among the class of rentiers. He puts forward an interesting but quite unsupported hypothesis that Bateson's early break with evolutionary embryology is connected with his rentier background and with the association of comparative embryology with the landed class through the person of F.M. Balfour and through the aristocratic nature of Balfour's College, Trinity.

largely channelled into defending its integrity and élite, anti-utilitarian ethos.⁽³¹⁾ His defence of traditional Cambridge was in spite of (or perhaps because of) the fact that his personal career in the University was largely unsuccessful. He never reached the prominent position of his father, and for a long time relied on marginal posts (such as the Stewardship of St. John's College) in order not to have to seek employment outside the University.

It is clear that several options were open to Bateson. He could, for example, have chosen to press for Cambridge University to 'move with the times', become 'relevant', and so on, and in doing so could have hoped that this would have increased his personal opportunities for advancement. (If he had taken this option he would indeed have been continuing the family tradition.) In adopting an anti-utilitarian, anti-reforming conservatism, he can, from the point of view of the sociology of knowledge at any rate, be seen as making a genuine choice.⁽³²⁾ Nevertheless, it was a choice between options that were themselves formed by the social structure. He was choosing to defend rather/...

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(31) On one issue Bateson was a 'progressive'. He was in favour of the admission of women to Cambridge degrees. Why he should have felt that this did not violate the Cambridge ethos, I do not know: it may be connected to the fact that his family contained several highly talented women.

(32) Of course, had we more information about his early life, psychological makeup and so on, we might no longer see this choice as free. The point, however, is that it would be mistaken to see it as constrained simply by his social background.

rather than reform, a given social institution. He was choosing opposition to, rather than furtherance of, a given process of industrialisation and modernisation. So it makes sense to see his conservatism as socially conditioned, as one response to a given set of social circumstances, even if not, at the level of Bateson as a concrete individual, socially determined. Although the generality of the conservative response is not crucial to this argument, it is interesting to note that Wynne (1977, 38-89) finds it to be prominent amongst Cambridge dons of Bateson's generation.⁽³³⁾

In general, then, the writings of Pearson and Bateson can be seen, I would suggest, as offering support to the sociological hypothesis put forward in the previous section. Their work can be taken as evidencing connections between ideas in evolutionary biology and social interests. The validity of the sociological hypothesis is not dependent on the correctness of the particular analyses of the writings of Pearson and Bateson put forward here: because the hypothesis involves no necessary claim about individuals, it could still hold even if these particular analyses should prove to be wrong. Nevertheless, the plausibility of the hypothesis is enhanced if it can help provide satisfactory accounts which deepen our understanding of individuals' work. As a further/...

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(33) See also Rothblatt (1968). It is interesting to contrast this conservative response with Pearson's call for Cambridge University to become more relevant and technologically-oriented (K. Pearson, 1886).

further test of its ability to do this, the evolutionary biology of R.A. Fisher will now be considered.

6.6 The Controversy Resolved? R.A. Fisher on Genetics and Evolution

Above, it was pointed out that the connection between the two issues of the validity of Mendelism and the nature of evolution was contingent, not necessary. The contribution of R.A. Fisher was to demonstrate this decisively, by showing the possibility and advantages of adopting a Mendelian view of heredity together with an orthodox Darwinian view of evolution. The general outline of how he did this is well-known (see, for example, Provine, 1971, 140-54), although the details are complex (Moran and Smith, 1966). In this section, I shall discuss one issue only: the relationship of Fisher's work to the ideological aspects of the controversy discussed above. (34)

The later controversy between Karl Pearson and R.A. Fisher/...

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(34) For an account of Fisher complementary to that given here, see Norton (1977). Similar analyses of the work of J.B.S. Haldane and Sewall Wright would be interesting. It may be (see below) that parallels between the approaches of Haldane and Fisher may be found; I should, however, be very much surprised if that were the case for Wright. The disagreements between the three founders of modern population genetics (for example, Fisher's violent dislike of Wright's notion of 'genetic drift') might possibly be illuminated by such an analysis.

R.A. Fisher has diverted the attention of historians from the strong similarities in overall perspective of the two men. Their family backgrounds were, broadly, similar. Fisher's father was a well-known auctioneer, from a family of businessmen. His maternal grandfather was a successful London solicitor. One of his uncles had been highly placed in the Cambridge Mathematics Tripos and had entered the church. He thus came from a family straddling the professional and commercial middle class, but one which does not seem to have been particularly wealthy. Family financial difficulties indeed forced Fisher to rely on scholarships for his university education (Mahalanobis, 1938). Like Pearson, Fisher was a convinced eugenicist. In conventional political terms he was certainly to the right of Pearson's Fabian socialism. Nevertheless, Fisher too can be interpreted as employing eugenics as an ideology of the professional middle class, if anything more explicitly. Thus, Fisher argued that the Eugenics Education Society should 'put itself in direct and sympathetic touch with the special aspirations of professional bodies' (1917, 212). A profession, he wrote (1917, 207):

... must have power to select its own members, rigorously to exclude all inferior types, who would lower both the standard of living and the level of professional status. In this process the eugenicist sees a desirable type, selected for its valuable qualities, and protected by the exclusive power of its profession in a situation of comparative affluence.

It was important that an 'exclusive profession' could 'offer advantageous prospects to the sons of its members', by, for example/...

example, 'requiring the nominations of each candidate by a number of members of the profession'. This would 'give a considerable advantage to the children of the professional men', and lessen the entry of 'new blood' which was 'on the whole, inferior to the professional families of long standing' and which rendered difficult 'the maintenance of a high tradition of professional etiquette' (1917, 210-11).

Fisher's eugenics was, like Pearson's, integrated into a wider social Darwinism.

From the moment that we grasp, firmly and completely, Darwin's theory of evolution, we begin to realise that we have obtained not merely a description of the past, or an explanation of the present, but a veritable key of the future. (1914, 309)

His interpretation of international competition, and of its relation to eugenics, was similar to Pearson's.

The modern nation is a genetic, territorial, and an economic organism ... European nations are grouping themselves along ethnic lines ... The widespread, fruitful, and successful races of the future belong to the dominant nations of today ... the overmastering condition of ultimate predominance is nothing else than successful eugenics; the nations whose institutions, laws, traditions and ideals, tend most to the production of better and fitter men and women, will quite naturally and inevitably supplant, first those whose organisation tends to breed decadence, and later those who, though naturally healthy, still fail to see the importance of specifically eugenic ideas.

(1914, 310-11)

Fisher identified the major eugenic threat as the high relative fertility of the 'socially lower classes', and warned that the low fertility of ruling classes had been the cause of the collapse of almost all past civilisations.

Fisher/...

Fisher was thus a convinced eugenicist and a convinced social Darwinist. There is no doubt that, in overall terms, he was more sympathetic to the biometric than to the Mendelian side of the biometrician/Mendelian debate; there is strong evidence from his early writings that this position of his is to be explained in terms of his eugenic and social Darwinist commitments. His first discussion of the matter (Fisher, 1911) explicitly treats biometry as a eugenic strategy:

Biometrics then can effect a slow but sure improvement in the mental and physical status of the population; it can ensure a constant supply to meet the growing demand for men of high ability.

'Mendelian synthesis', by comparison, promised quick and 'almost miraculous' results, but Fisher appears to have doubted the practicality of its application to man, dependent as it was on 'experimental breeding'. In a later paper, written jointly with his fellow Cambridge eugenicist C.S. Stock, Fisher noted the existence of a 'confused controversy' between Darwinians and 'extreme Mendelians'. Fisher and Stock argued:

It is essential for Eugenists to consider on which side they ought to range themselves ...
(1915, 60)

Closely echoing Pearson, they argued that 'it is in the highest degree unlikely that Mendelism will ever cover even the field of heredity' (1915, 60) and that Mendelism was being rashly applied:

... regrettable things have been done, and more regrettable things have been said in America in the name of Mendel. Direct legislative proposals have been made, and in some cases passed, based upon quite/...

quite inadequate knowledge. Persons suffering from supposedly Mendelian defects have been advised to mingle with sound stocks, though the result of doing so is clearly to lay up hereditary trouble for the future.
(1915, 59)

Eugenists were thus 'open to all kinds of attack on the side of Mendelism'. By comparison, 'on Darwin's ground they are impregnable'.

Were all information, except that used by Darwin inaccessible, such information would not only allow but compel us to formulate eugenic concepts and proposals. Changes in the constitution of a mixed population depend primarily upon selection; the existing and possible agencies of selection do at present and must always provide the most fruitful field of eugenic research. These agencies acting at large amidst a multitude of random causes, each of which may have predominant influence if we fix our attention upon a particular individual, nevertheless determine the progress or decadence of the population as a whole. We may borrow an illustration from the kinetic theory of gases. The several molecules are conceived to move freely in all directions with greatly varying velocities, but the statistical result is a perfectly definite measurable pressure. Controversy may rage round the nature and properties of the atom, yet our knowledge of general principles enables us to calculate gas pressures with accuracy. We are independent of particular knowledge about separate atoms, as in eugenics we are independent of particular knowledge about individuals. It is by no means suggested that such knowledge is not of the highest importance, interest and use. It is, however, unnecessary alike for a general theory of gases and for a general theory of eugenics.
(Fisher and Stock, 1915, 60-1)

It would seem, then, that Fisher's scientific judgments, like those of the biometricians, were informed by a cognitive interest in controlling and predicting the overall incidence of characteristics in populations. Further, it appears that in his case, too, this cognitive interest can be linked to a desire to construct a social evolutionism that/...

that would form a solid basis for prediction and control of the evolution of societies, and, more particularly, for eugenics. The development of a theory of natural selection expressing this cognitive interest was precisely what Fisher achieved in the Genetical Theory of Natural Selection. Take, for example, Fisher's 'fundamental theorem of natural selection' (1930, 35; emphasis deleted):

The rate of increase in fitness of any organism at any time is equal to its genetic variance in fitness at that time.

This theorem, Fisher argued, enabled one to dismiss the objection that 'natural selection depends on a succession of favourable chances'. Natural selection did operate according to the laws of chance, but according to their 'continuous and cumulative action' (1930, 37); when evolution was conceived of as a mass process, laws of evolution could indeed be formulated. In the Genetical Theory one can still see the traces of the connection of this cognitive interest to eugenics and social Darwinism. Five of the book's twelve chapters deal with social evolution and eugenics, and Fisher comments in the introduction (1930, x):

The deductions respecting Man are strictly inseparable from the more general chapters ...

Indeed, Fisher argued that detachment from the outcome 'in the real world' of theoretical researches such as his was improper:

Such detachment sterilises theory as much as it blinds practice.
(1930, 264)

In/...

In the light of this cognitive interest, it is not surprising that Fisher's assessment of the evolutionary theories of Bateson was negative. Bateson's influence upon evolutionary theory was 'chiefly retrogressive', his early writings were 'rash polemics' (1930, ix-x). Even in his Cambridge days, Fisher was 'consciously out of sympathy' with the tendency in evolutionary theory represented by Bateson (Mahalanobis, 1938, 239). Fisher's attitude to Bateson's genetics, however, was quite different. In Fisher (1911), he told the Cambridge University Eugenics Society of 'the view of inheritance which I have taken up':

On this theory the inherited nature of any living creature consists of a large number of Mendelian characters ...

Despite his qualms about the consequences for eugenics of 'extreme Mendelism', he adhered from then on to a Mendelian theory: arguing that it was not incompatible with the observed biometric distributions and correlations (1911); using a multifactorial Mendelian model to produce the first heritability estimate (1918a); claiming that 'Mendelism supplied the missing parts of the structure first erected by Darwin' (1930, ix).

From the very start of his work Fisher realised that the issues of the biometrician/Mendelian debate were separable. As he and Stock put it in 1914:

Darwinism is concerned with evolution, Mendelism with the mechanism of heredity.
(59)

Fisher took up the path that Pearson had opened with his papers/...

papers on Mendelism, but had never taken: that of accepting Mendelism and adapting it as a resource for the development of eugenics and a theory of evolution reflecting the cognitive interests of the biometricians. Fisher did not obtain support from Karl Pearson in doing so: neither (despite the generally accepted historical view of the refereeing of Fisher, 1918a) did he face active opposition or hostility from Pearson.⁽³⁵⁾ He did, however, clash several times with Bateson's collaborator, R.C. Punnett (1875-1967).

There are interesting similarities between Punnett and Bateson. Although Punnett, who became first Professor of Genetics at Cambridge, was born to a less elevated position in the middle class than Bateson, he married into the landed gentry (Crew, 1967, 317). Like Bateson, he was a leader of the campaign to defend compulsory Greek (Crew, 1967, 315). His biological and social views, and the intermingling of the two, were to a degree similar to those of Bateson, as when he wrote (Punnett, 1925, 707):

The general character of a population is a very stable thing, highly resistant to external influences, and marked changes in its inherent nature are to be measured only in terms of centuries or tens of centuries. But it may be objected that progress is rapid, and its effects are striking in periods far shorter than the span of a man's life. To which the biologist would reply that such progress is not dependent upon any change in the constitution of the population itself, but to the appearance in it of a few individuals/...

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(35) See Norton and Pearson (1976, especially 153-4).

individuals endowed with exceptional qualities. Those who want more progress must see to it that more of these exceptional minds are produced and given their opportunities.

Punnett, like Fisher, felt the danger of 'the intellectual and professional classes' being crushed 'between the upper and nether millstones of the commercial class and the proletariat' (1926, 84-6). This did not, however, lead him to the self-confident interventionism of Fisher's eugenics. Punnett played an important role, instead, in pointing to the limitations of eugenic intervention, by showing that deleterious recessives could not be 'bred out' of the population as quickly as the eugenists hoped (Punnett, 1917).

The disagreements between Fisher and Punnett recapitulated many of the issues of those between Pearson and Bateson. Most prominently, they disagreed over the question of the nature of evolution. Thus, Fisher (1927; 1930, 146-69) disputed the interpretation of mimicry in butterflies put forward by Punnett (1915). Punnett had given a Mendelian account of mimicry and argued that it was 'an evolutionary phenomenon which must occur by distinct leaps' (Provine, 1971, 137). Fisher argued that the validity of a Mendelian account of mimicry did not imply its origin by saltation, because the phenotypic effects of a gene 'may be influenced, apparently to any extent, by means of the selection of modifying factors' (1930, 166): instead, Fisher defended the thesis of the 'gradual evolution of such mimetic resemblances'. Punnett, in his turn, took issue with Fisher's Genetical Theory of Natural Selection on account of/...

of its evolutionary gradualism and selectionism, claiming a much more important evolutionary role for mutations than that allowed by Fisher (Punnett, 1930). Other disagreements concerned the efficacy of eugenic intervention - Fisher (1924) taking issue with Punnett for having 'inadvertently supplied material for anti-eugenic propaganda' - and the general importance and relevance to biology of mathematical work such as Fisher's. (36)

It is thus only if one takes a narrow view of the biometrician/Mendelian controversy that Fisher can be seen as providing a resolution of it. Fisher is, I would argue, better seen as continuing the controversy, albeit with somewhat different weapons. The cognitive interests manifested in his work, and their connection with social interests, are essentially the same as in the work of Pearson. Because of this, and because of the particularly explicit connection between Fisher's eugenics, his social Darwinism, and his perception of the social interests of the professional middle/...

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(36) In his referee's report on Fisher (1918a), Punnett commented:

... I do not feel that this kind of work affects us biologists much at present. It is too much of the order of problem that deals with weightless elephants upon frictionless surfaces...
(Norton and Pearson, 1976, 155)

middle class, the case of Fisher provides further evidence for the hypothesis suggested in section 6.4.⁽³⁷⁾

The Controversy over the Measurement of Association

The controversy between Karl Pearson and his followers, on the one hand, and George Udny Yule, and a small group of supporters, on the other, about how best to measure association is much less well-known, but in some ways no less bitter, than that between the biometrists and the Mendelians. I shall argue that it too can be analysed in roughly the same fashion. Despite the detailed and esoteric nature of the issues at stake, social interests may be important in

- (37) The work of Haldane on evolutionary theory (see, for example, Haldane, 1932) is not considered here. One point that may, however, be worth making, is that Haldane was not as different in political terms from Pearson (or Fisher) as might be supposed. At approximately the same time as Fisher was active in the Cambridge University Eugenics Society, Haldane joined the committee of the newly formed Oxford Society and advocated the eugenic case in an Oxford Union debate (Searle, 1976, 13). Although he left the Eugenics Society after 1920 (Werskey, 1971, 179), he retained a basic sympathy for much of the eugenic case:

... Pearson and his colleagues were completely right in one respect. Even if, in spite of his predictions, the nation has improved in some measurable directions, it would have improved more if, say, a million children who were born to unskilled labourers had been born to skilled workers, teachers, and the like. (Haldane, 1957, 435)

Like Pearson, Haldane believedⁱⁿ a "scientific society" ruled by an enlightened élite' (Werskey, 1971, 178). His comments on Pearson (Haldane, 1957, especially 437) show clearly the extent to which he saw both his science and his politics as in continuity, not conflict, with Pearson's.

Chapter Seven

The Controversy over the Measurement of Association

The controversy between Karl Pearson and his followers, on the one hand, and George Udny Yule, and a small group of supporters, on the other, about how best to measure association is much less well-known, but in some ways no less bitter, than that between the biometricians and the Mendelians.⁽¹⁾ I shall argue that it too can be analysed in roughly the same fashion. Despite the detailed and estoric nature of the issue at stake, social interests may be important in this case also.

I begin by describing the two publications in 1900 in which Pearson's and Yule's divergent views on association were first presented. In sections 7.2 and 7.3, I discuss the further development of their views and their evaluations of each other's position. I then argue that the theorising and scientific judgments of Pearson and Yule have to be understood as embodying different cognitive interests. In section 7.5, these cognitive interests are related to the different 'goal orientations' of Pearson and Yule: that is to/...

- - -

- (1) There has been no full historical analysis of the controversy over the measurement of association. Walker (1929, 130-41) gives a useful annotated bibliography and the important theoretical work by Goodman and Kruskal (1954-9) contains a thorough review of work on association.

to the different objectives which can be seen as conditioning their work in statistical theory. The analysis is then extended in section 7.6 to include the other members of the British statistical community who supported one or other of the two leading participants, and possible alternative explanations of the controversy are discussed. The chapter ends by referring back to the analyses presented in chapters three, four and six, in order to suggest, tentatively, a way in which social interests can be seen as bearing on this controversy.

7.1 The Issue

By 1900 British statisticians had reached apparent consensus on how to measure the correlation of those variables, such as height and weight, for which a measurement scale with a valid unit of measurement existed. In his concepts of regression and correlation Francis Galton had provided the basic technology for dealing with these 'interval' variables.⁽²⁾ F.Y. Edgeworth, S.H. Burbury and Karl Pearson had extended the theory from two to any number of variables, and Pearson had provided the now standard product-moment formula for the coefficient of correlation. Aside from some private disagreement⁽³⁾ as to the extent to which/...

- - -

(2) The use of terms such as 'interval' and 'nominal' here is anachronistic, but their use clarifies the issue at stake. For these terms see S.S. Stevens (1946).

(3) This disagreement is discussed below in section 7.4.

which Galton's theory, developed for normally-distributed variables, could be applied to non-normal variables, the problem seemed solved for interval-level variables. From 1900 onwards attention shifted to nominal variables - those in which no unit of measurement was available, and classification into different categories was all that was possible. The two main attempts to develop a theory of the association of nominal variables were by Karl Pearson and George Udny Yule (1871-1951).

Let us consider Yule's work (Yule, 1900) first. His approach was extremely direct. Consider a set of N objects, classified according to two nominal variables A and B . Each object is classed as either A_1 or A_2 , and either B_1 or B_2 .⁽⁴⁾ Thus A_1 might be 'survived an epidemic', A_2 'died in the epidemic'; B_1 'vaccinated', B_2 'non-vaccinated'. The data can be presented conveniently as follows:

| | B_1 (vaccinated) | B_2 (unvaccinated) | Total |
|------------------|--------------------|----------------------|---------|
| A_1 (survived) | a | b | $a + b$ |
| A_2 (died) | c | d | $c + d$ |
| Total | $a + c$ | $b + d$ | N |

Thus/...

- - -

- (4) In the following I have been forced, for the sake of clarity, to use a standard form of notation. This is to be regretted, as Yule's and Pearson's notations did to some extent reflect their differing approaches. Yule used a notation drawn from symbolic logic. For A , and A_2 he wrote A and α , where α signified not- A , and for B , and B_2 he wrote B and β , with β signifying not- B . His notation for the frequency I label 'a' was (AB) , for 'b', $(A\beta)$, etc.

Thus 'a' is the number of those vaccinated who survived the epidemic, 'b' of those unvaccinated who survived the epidemic, and so on.

Yule argued that a coefficient of association for such a table must have three properties. Firstly, it should be zero if and only if A and B are non-associated or independent. In the above example, survival and vaccination (A and B) would be said to be independent if the proportion of survivors was the same amongst the vaccinated and the unvaccinated. This can be expressed symbolically as:

$$\frac{a}{a + c} = \frac{b}{b + d}$$

$$\text{or } ab + ad = ab + bc$$

$$\text{or } ad - bc = 0.$$

Working backwards through this chain of thought, it can be shown that $ad - bc = 0$ implies that A and B are non-associated. Thus the first desideratum will be satisfied by a coefficient which has the value zero if and only if $ad - bc = 0$.

The second property is that the coefficient should be +1 when, and only when, A and B are completely associated. There are two possible senses of complete association here. The first is the strong sense in which A and B are said to be completely associated only when all A_1 's are B_1 's and all A_2 's are B_2 's (i.e. $b = c = 0$). In the above example, this would mean that all those who were vaccinated survived and all those who were not vaccinated died. There is also a weaker/...

weaker sense of complete association, according to which A and B are completely associated if either all A_1 's are B_1 's or all A_2 's are B_2 's. Either of the following two tables thus displays complete association in this sense:

| | B_1 (vaccinated) | B_2 (unvaccinated) |
|------------------|--------------------|----------------------|
| A_1 (survived) | a | 0 |
| A_2 (died) | c | d |

| | B_1 (vaccinated) | B_2 (unvaccinated) |
|------------------|--------------------|----------------------|
| A_1 (survived) | a | b |
| A_2 (died) | 0 | d |

In the first table none of the unvaccinated survive (even though some of the vaccinated die). In the second none of the vaccinated die (even although some of the unvaccinated live). Yule chose to use this weaker definition of complete association; thus, his second criterion was that the coefficient should be +1 if and only if either $b = 0$ or $c = 0$.

The third property is that the coefficient should be -1 when A and B are completely associated in a negative sense. Again, there is a strong and a weak meaning of complete negative association, and Yule chose the weak meaning. A and B are completely associated in the negative sense when either all A_1 's are B_2 's or all A_2 's are B_1 's.

| | B_1 | B_2 |
|-------|-------|-------|
| A_1 | 0 | b |
| A_2 | c | d |

or

| | B_1 | B_2 |
|-------|-------|-------|
| A_1 | a | b |
| A_2 | c | 0 |

Thus the coefficient should be -1 if and only if either $a = 0$ or $b = 0$.

Yule then examined the coefficient $Q = \frac{ad - bc}{ad + bc}$.

Clearly, if $ad - bc = 0$, then $Q = 0$. Conversely $Q = 0$ implies $ad - bc = 0$. So Q satisfies the first condition. If either $b = 0$ or $c = 0$, then $bc = 0$, and $Q = ad/ad = +1$. Also if $Q = +1$, then $ad - bc = ad + bc$, hence $bc = 0$, and so either $b = 0$ or $c = 0$. So Q satisfies the second condition. Finally, if either $a = 0$ or $d = 0$, then $ad = 0$, and $Q = -bc/bc = -1$; conversely $Q = -1$ implies $ad - bc = -ad - bc$, hence $ad = 0$, and so either $a = 0$ or $d = 0$. Q thus satisfies all three conditions, and Yule put it forward as a measure of association in two-by-two tables. However, as Yule was aware, Q has no special justification. There are an unlimited number of functions which satisfy Yule's three conditions - for example Q^3 , Q^5 , and so on. Further, as Pearson was later to show, two different tables could be ranked in one order as regards strength of association by one of these functions, and in a different order by another.

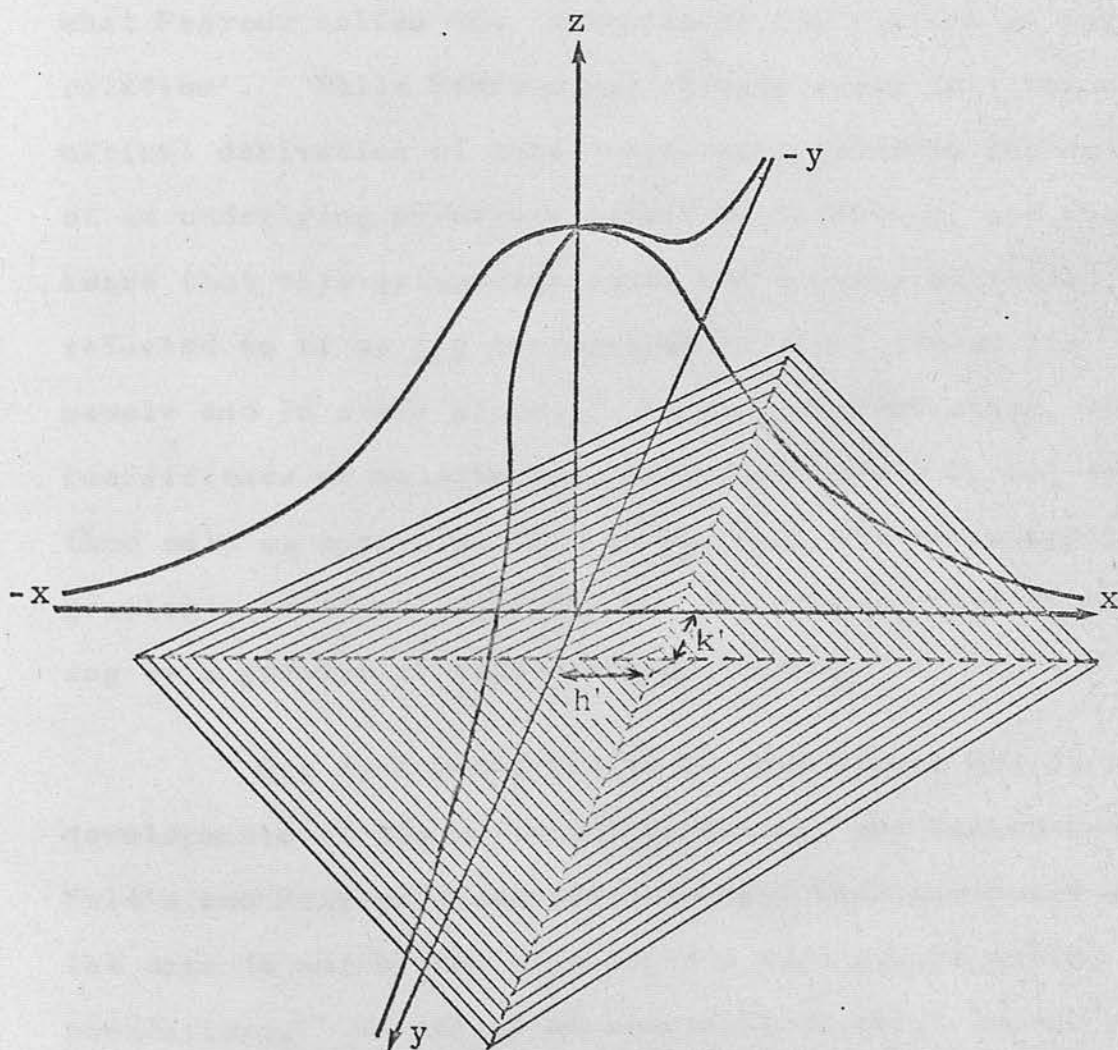
Pearson's approach (Pearson, 1900b) was to produce, by a much tighter but more precarious theoretical argument, a/...

a coefficient of association which he called the 'tetrachoric coefficient of correlation'. I shall denote it by r_T (Pearson's denoted it simply by r). The crucial assumption at the base of the derivation of r_T is that the observed four-fold table can be regarded as having arisen in the following fashion. The observed categories A_1 , A_2 and B_1 , B_2 are taken to correspond to ranges of more basic interval variables y and x : A_1 corresponding, for example, to $y \leq k'$, A_2 to $y > k'$, B_1 to $x \leq h'$, B_2 to $x > h'$. It is further assumed that y and x jointly follow a bivariate normal distribution, with x having zero mean and standard deviation σ_1 , and y zero mean and standard deviation σ_2 . Geometrically this can be shown as in figure two. In figure two we see the bivariate normal frequency surface (which is shaped like a bell with elliptical cross-sections) rising above the plane of x and y . This plane is divided into four quadrants by lines through the point (h', k') - the four quadrants corresponding to the cells of the four-fold table. The volume above the top left of these quadrants corresponds to the frequency with which $x \leq h'$ and $y \leq k'$, and thus corresponds to the frequency a in the original table.

Pearson had thus provided a model of a statistical distribution assumed to underly the given two-by-two table. The model has three parameters, h'/σ_1 , k'/σ_2 , and r , the correlation of x and y . There are three independent parameters in the given table (not four, as the total, N , is regarded as fixed and $a + b + c + d = N$). The model can be fitted/...

Figure 2

Pearson's Model of Underlying Variables



fitted to any four-fold table, as the equations relating the model and the observations are always soluble, although the solution requires the use of numerical methods (see appendix D). A value for r , the correlation of the underlying variables, can thus be found.

Approaches

This correlation of the underlying variables was what Pearson called the 'tetrachoric coefficient of correlation'. While Pearson was clearly aware that the mathematical derivation of this coefficient involved the assumption of an underlying bivariate normal distribution, and was also aware that this assumption could not usually be tested, he referred to it as the correlation in the title of his memoir and in other places. He did consider other, empirical, coefficients of association, including Yule's Q , but treated them only as approximations to r_T , with the advantage of much greater ease of calculation, but the disadvantage of deviating by a greater or lesser extent from r_T .

One last point has to be made before the further developments of the different approaches are considered. Yule's and Pearson's coefficients have been presented as if the data to which they were applied were always entire populations. In this I am remaining faithful to the work of Yule and Pearson, who did not systematically distinguish between sample statistics and population parameters. Yule and Pearson were of course aware that the data to which they applied Q and r_T were often drawn from samples, but, apart/...

apart from calculating the 'probable errors' of their coefficients, they did not address themselves generally to the problems posed by this.

7.2 Further Developments In Pearson's and Yule's Approaches

The invention of the tetrachoric coefficient by no means concluded Pearson's theoretical work on the measurement of association. Indeed, this area was a major focus of his work in mathematical statistics from 1900 to 1922. Pearson was fully aware of the shortcomings of r_T - in particular, its restriction to two-by-two tables. While continuing to champion the use of r_T , he attempted to find an approach to the problem of the measurement of association that would allow the direct analysis of larger tables (those in which objects are classed as A_1, A_2, \dots, A_p and B_1, B_2, \dots, B_q) and would, if possible, avoid the assumptions involved in the derivation of r_T .

The most important of these attempts was his development of the theory of contingency. This derived from the application of his own χ^2 test to two-way tables (Pearson, 1900c). For any such table it is possible to work out the expected frequencies in each cell on the assumption that the two variables are independent, and then to measure the divergence between observed and expected frequencies by means of χ^2 . Reference to the distribution of χ^2 then gives a/...

a measure of the probability of such a divergence from the expected frequencies, on the assumption of independence. The value of χ^2 itself was of little direct interest to Pearson. He wanted not simply to reject the hypothesis of no association, but to measure the strength of association. The value of χ^2 cannot serve as such a measure, because multiplying the frequencies in each cell of a table by a constant (which presumably does not alter the strength of association) multiplies the value of χ^2 by that constant. This problem is, however, easily avoided. If the value of χ^2 is divided by N , the total number of cases in the table, then the resultant coefficient clearly remains unaltered by multiplication of each cell in the table by a constant. This coefficient $\phi^2 = \chi^2/N$ Pearson (1904b, 6) referred to as the mean square contingency.

A measure based on χ^2 has clear attraction. It is free from any need to assume underlying variables, and it can be applied to any size of table. It is even independent of the ordering of the categories of each variable. The problem is, which particular measure based on χ^2 should be used? Once again, Pearson solved this problem by reference back to the correlation of normally distributed interval variables. He supposed any given table to have arisen by splitting these continuous variables into categories. He then found a relationship between the mean square contingency for such a table and the coefficient of correlation of the underlying variables, r . In the limiting case that the/...

the number of cells in the table tends to infinity, he showed (Pearson, 1904b, 7-8) that:

$$r = \pm \sqrt{\frac{\phi^2}{1 + \phi^2}}$$

He then proposed the coefficient:

$$C_1 = \sqrt{\frac{\phi^2}{1 + \phi^2}}$$

which he called the 'first coefficient of contingency' (Pearson, 1904b, 9).⁽⁴⁾ If the two-way table had arisen by categorisation of an underlying bivariate normal distribution, and if the number of cells in the table was large, then C_1 approximated to the coefficient of correlation of the underlying variables. Because C_1 is a monotonic function of the value of χ^2 for the table from which it is calculated, it has also a certain justification quite apart from the validity of these assumptions.

C_1 did not displace r_T in Pearson's affection. Pearson felt that C_1 was best used only in larger tables (of about 25 cells), because for small tables the limit relationship between C_1 and r did not hold, and thus C_1 was a bad estimate of the correlation of underlying variables.

Hence the new conception of contingency, while illuminating the whole subject ... does not do away with the older method of fourfold division. (Pearson, 1904b, 9)

- - -

- (4) Pearson also proposed a second coefficient of contingency, based on a different function of the divergence between observed and expected frequencies. This was easier to calculate but did not have any similar clear relationship to r , and was less used.

Pearson's fundamental criterion was still the relationship between a coefficient of association and the correlation of underlying variables: he still sought a coefficient of association directly comparable with the correlation coefficient of interval variables.

Other developments of the theory of association by Pearson and his co-workers follow broadly on the same lines.⁽⁵⁾ The desire for comparability with the interval-level coefficient of correlation can be seen in such comments as 'in order that our results shall agree fairly closely with the results for Gaussian distributions we select ... our scale ...' (Pearson, 1912a, 24). One major aim of this work was to 'improve' C_1 by various corrections, the most important being the class-index correction, described in Pearson (1913a). Again, the basis of the correction is the assumption of underlying continuous variables, and the purpose of the correction is to improve the estimate of the correlation of these variables by taking account of the fact that C_1 is calculated from a finite number of cells rather than the infinite number presupposed by the limit relationship between C_1 and r . Uncorrected, C_1 has a tendency to underestimate the 'true' correlation. The typical effect of a class index correction on a five-by-five table is to boost C_1 by about 0.05.

The final attempt Pearson made to find a 'perfect' solution/...

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(5) See, for example, Pearson (1910a; 1912a; 1913).

solution to the problem of the measurement of association was to derive an iterative method for fitting a bivariate normal distribution to a two-way table (in effect, to find a counterpart to r_T for tables larger than two-by-two). A solution to this problem was published in a joint paper with his son Egon Pearson (K. and E.S. Pearson, 1922). But the resultant 'polychoric coefficient', while representing in a sense the logical conclusion of Karl Pearson's approach to the problem, was in that pre-computer age defeated by the sheer laboriousness of its mode of calculation.

Yule developed two further coefficients, the 'product-sum coefficient', r_{PS} , and the 'coefficient of colligation', w . These two coefficients did not represent any fundamental break with the approach lying behind his earlier work. Both satisfy his three criteria for a coefficient of association, with the only difference being that, while Q and w take the value 1 for perfect association in the weak sense (either b or c zero), r_{PS} takes this value only for positive association in the strong sense (both b and c zero). The product-sum coefficient is the ordinary interval-variable coefficient of correlation applied to a two-by-two table, not on Pearson's sophisticated model, but 'naïvely', by making the assumption that the two categories correspond to the values 0 and 1 of a discrete variable. It can be shown that this yields the value:

$$r_{PS} = \frac{ad - bc}{\sqrt{(a+c)(b+d)(a+b)(c+d)}}.$$

Yule/...

Yule referred to r_{PS} as 'the correlation-coefficient for a [two-by-two] table' although he did not suggest it displaced Q .⁽⁶⁾ The coefficient of colligation (Yule, 1912)

links Q and r_{PS} . The formula for it is

$$w = \frac{\sqrt{ad} - \sqrt{bc}}{\sqrt{ad} + \sqrt{bc}}$$

and Q and w are related by a simple equation:

$$Q = \frac{2w}{1 + w^2}.$$

When the given two-by-two table is reduced to a standardised symmetrical form by multiplication and division of the rows and columns by constants until each marginal total equals $\frac{1}{2}N$, w for the original table equals r_{PS} for the standardised table. So w and r_{PS} are also related. But the inter-relatedness of Q , w and r_{PS} is much weaker than the inter-relatedness of Pearson's coefficients, all of which bear some reference to the single theoretical standard of the interval-variable coefficient of correlation. Q , w , and r_{PS} give different values when applied to the same table, and Yule gave no general rules as to which to use in a given case.

7.3 The Controversy

The/...

- - -

- (6) The product-sum coefficient was first introduced by Yule (1911, 212-3). This coefficient had previously and independently been suggested by the geneticist W. Johannsen (1909, 272-9) and by the anthropologist F. Boas (1909). It had even been used by Pearson (1904a) quite without comment, but in a very different situation, that of theoretical Mendelian inheritance (for which see below).

The fundamental issues at stake in the controversy were implicit in the two original papers that Pearson and Yule published in 1900. Neither openly attacked the other, however, and personal relations between the two men seem to have remained good. Open conflict began only in late 1905. On 7 December, Yule read to the Royal Society of London two papers (Yule, 1906a; Yule, 1906b) critical of some aspects of Pearson's work, in particular throwing doubt on the validity of the assumptions underlying Pearson's use of the tetrachoric coefficient. Pearson replied to these criticisms in an article in Biometrika (Pearson, 1907). At this stage, the controversy was still not generalised to all aspects of the competing approaches to the measurement of association. This happened only when Yule published his textbook An Introduction to the Theory of Statistics (1911), in which he gave an account of his measures Q and r_{PS} . Pearson's collaborator David Heron wrote a sharply-worded warning to the readers of Biometrika on the 'danger' of Yule's formulae (Heron, 1911). Yule in his turn read to the Royal Statistical Society a long paper defending his position and attacking Pearson's (Yule, 1912). Pearson and Heron replied in a paper covering 157 of the large pages of Biometrika (Pearson and Heron, 1913; see also Pearson 1913b). This paper effectively marked the end of the overt phase of the controversy (though see also Greenwood and Yule, 1915). It was, however, unresolved. Pearson and Yule no doubt felt they had fully stated their positions, but neither had succeeded/...

succeeded even partially in convincing the other. Yule's obituary notice of Pearson refers to the controversy and comments, 'Time will settle the question in due course' (Yule, 1936, 84).

The main focus of Yule's attack on the tetrachoric coefficient was on the assumptions involved in its derivation and use. He wrote (Yule, 1912, 140):

The introduction of needless and unverifiable hypotheses does not appear to me a desirable proceeding in scientific work.

When dealing, for example, with vaccination statistics (an area where biometricians had applied the tetrachoric method), Yule argued that 'vaccinated', 'unvaccinated', 'survived' and 'died' constitute naturally discrete classes:

... all those who have died of small-pox are all equally dead: no one of them is more dead or less dead than another, and the dead are quite distinct from the survivors.
(Yule, 1912, 139-40)

To apply here a coefficient that had as its basis an assumption of underlying continuous variables was absurd:

At the best the normal coefficient can only be said to give us in cases like these a hypothetical correlation between supposititious variables.
(Yule, 1912, 140)

There were cases, Yule conceded, where the assumption of underlying continuity was 'less unreasonable'. In these cases, however, the hypothesis that the underlying distribution is bivariate normal was frequently doubtful. Pearson had often used the tetrachoric coefficient in two-by-two tables which had been obtained from larger tables by the/...

the amalgamation of adjacent classes. Indeed until his invention of the coefficient of contingency he was forced to do this, as he had no method of analysing larger tables. In these larger tables, unlike two-by-two tables, it was possible to test the validity of the hypothesis of an underlying bivariate normal distribution.

This could be done in two ways. First, if the hypothesis is true, then it should not matter, from the point of view of the calculation of r_T , which precise way one chose to amalgamate classes. The value of r_T should be at least approximately independent of the boundary line chosen between the two final classes. Yule was thus able to test Pearson's hypothesis by calculating r_T in several different ways for the same large table. He showed (Yule, 1912, 144) that, at least in certain cases given by Pearson, the values obtained varied considerably, ranging for example from 0.27 to 0.58 in a table on the resemblance between fathers and sons in eye-colour. Secondly, if a large table has in fact arisen according to Pearson's hypothesis, then it should display the property Yule termed 'isotropy'. Consider any 4 adjacent frequencies, n_1 , n_2 , n_3 and n_4 extracted from a larger table.

| | | | | |
|--|--|-------|-------|--|
| | | | | |
| | | | | |
| | | n_1 | n_2 | |
| | | n_3 | n_4 | |
| | | | | |

The/...

The table is called 'isotropic' if the sign of $n_1n_4 - n_2n_3$ is the same for all similar 'sub-squares' of the table. In his first published criticism of Pearson's work, Yule tested for 'isotropy' tables on which Pearson had, after amalgamation of classes, used r_T . He found (Yule, 1906a) that many were not 'isotropic'.

Pearson (1907) defended himself by arguing that Yule's isotropy criterion was invalid because he had failed to evaluate the probable error of $n_1n_4 - n_2n_3$. Because a given table is only a sample from a larger population, a failure of isotropy may occur through random fluctuation alone. Pearson accepted that the variation in values of r_T obtained in different ways from the same table showed that in certain cases the assumption of underlying normality did not appear to be tenable. But he had been aware of this, he said, and the method of contingency had been developed to deal precisely with those cases. When coefficients of contingency were worked out for the tables in question, they were found to agree 'sensibly' with the tetrachoric coefficients, and Pearson claimed that his conclusions thus held, despite the flaws in the method by which they had been obtained.

The basis of the attack on Yule's approach mounted by Pearson and Heron was that, for the same table, Yule's various coefficients did not agree in value, and further that for tables formed from genuine bivariate normal data none/...

none agreed with the ordinary correlation coefficient. For one table given by Yule, Heron found that $Q = 0.91$ while $r_{PS} = 0.02$. For bivariate normal data, Q did not differ very much from the correlation coefficient so long as divisions were taken near the medians, but for more extreme divisions the divergence could be large (e.g. $r = 0.5$, $Q = 0.97$). For such data, Q varied in value according to exactly where the divisions were taken; the same is true of r_{PS} (and indeed of w).

Pearson and Heron felt that Yule was reifying his categories. Only in rare cases - such as that of Mendelian theory, where the categories of a two-by-two table correspond to the presence or absence of a Mendelian unit and thus the two variables genuinely are discrete (factor present = 1; factor absent = 0) - was the use of such methods justified. In these cases r_{PS} was the correct way to extend the ordinary theory of correlation, as it assumed just such discrete variables. In general, however, treating categories in this way was mere empty formalism.

And here we will at once emphasise the fundamental difference between Mr. Yule and ourselves. Mr. Yule, as we will indicate later, does not stop to discuss whether his attributes are really continuous or are discrete, or hide under discrete terminology true continuous variates. We see under such class-indices as 'death' or 'recovery', 'employment' or 'non-employment' of mother, only measures of continuous variates - which of course are not a priori and necessarily Gaussian ...

The controversy between us is much more important than an idle reader will at once comprehend. It is the old controversy of nominalism against realism/...

realism. Mr. Yule is juggling with class-names as if they represented real entities, and his statistics are only a form of symbolic logic. No knowledge of a practical kind ever came out of these logical theories. As exercises for students of logic they may be of educational value, but great harm will arise to modern statistical practice, if Mr. Yule's methods of treating all individuals under a class-index as identities become widespread, and there is grave danger of such a result, for his path is easy to follow and most men shirk the arduous.

(Pearson and Heron, 1913, 161, 302)

Pearson and Heron justified the biometric position by arguing that it was necessary to make some hypothesis about the nature of the continuous frequency distribution of which the observed classes were groupings. The only distribution which had been adequately studied mathematically was the normal. In practice, they argued, methods based on the normal distribution almost always gave adequate results. The unique advantage of these methods seemed to them to outweigh the difficulties involved:

The coefficient of correlation has such valuable and definite physical meanings that if it can be obtained for any material, even approximately, it is worth immensely more than any arbitrary coefficients of 'association' and 'colligation'.
(Pearson and Heron, 1913, 300)

7.4 Cognitive Interests

In a very general sense the work of Pearson and Yule can be seen as manifesting the same cognitive interests. In providing measures of association, both Pearson and Yule were attempting to extend the scope of scientific prediction into a field where no reliable techniques of inference were available/...

available. To frame matters like this is, however, insufficiently specific. There was no single 'natural' way to extend the scope of statistical analysis into this new area; the different ways in which Pearson and Yule did it can perhaps be accounted for by the differing concrete forms in which general interests in prediction and control were manifested.

Pearson's work was dominated by its reference to an existing achievement of statistical theory, the interval-level theory of correlation and regression. For Pearson, this theory was an exemplary instance of the way statistics enhanced the scope of prediction. Thus, regression was the theory of how best to predict the value of one variable from that of another, in situations where there was no one-to-one correspondence. The correlation of two variables was, for Pearson, that constant, or set of constants, that was sufficient to describe how the expected value of one variable depended on the value of another (Pearson, 1896, 256-7). In one case only had the correlation in this sense been fully specified: that of two variables that followed a bivariate normal distribution. Given the correlation coefficient for two such variables, it was possible to state immediately the expected value of one variable associated with any value of the other.

Pearson's approach to the association of nominal variables was evidently structured by an interest in maximising/...

ing the analogy between the association of such variables and the correlation of interval-level variables with a joint normal distribution. This correlation had a clear meaning in terms of prediction, and this meaning made it uniquely suitable as the criterion for judging the strength of association. Use of this basic reference point was the foundation of Pearson's attempt to construct a unitary theory of association and correlation, and of his negative evaluation of the work of Yule.

The derivation of r_T shows that Pearson initially defined association as the correlation of the hypothetical underlying bivariate normal distribution. In the later work on contingency this literal superposition of the two cases was partially discarded: Pearson accepted that the assumption of an underlying bivariate normal distribution might not be factually correct. But the analogy still operated, as can be seen in the way that the bivariate normal model was used to choose the particular functions of χ^2 that were selected to be the coefficients of contingency. Measures of association were thus seen by Pearson as ways of estimating the correlation of an actual or notional underlying distribution. This was, in effect, simply what Pearson meant by 'measuring association', and the way in which he described r_T as 'the coefficient of correlation' indicates the taken-for-granted nature of the metaphor. For Pearson, the basic criterion of the validity of coefficients of association was their usefulness in the estimation/...

estimation of this underlying correlation.

This criterion of validity was typically operationalised in the following way. Interval data that followed a bivariate normal distribution would be taken, and from this data a two-by-two or larger table would be constructed. Thus, if the data referred to the height and weight of individuals, a two-by-two table could be constructed by classifying those individuals over six feet as 'tall', those under as 'short', those over 150 lb. as 'heavy', those under as 'light'. A coefficient of association would then be applied to this table. If the value of the coefficient approximated well to the interval-level correlation of height and weight, this was a point in its favour. If the values of a coefficient did not tally with the coefficient of correlation, then this was an argument for its rejection.

The tetrachoric coefficient passed this test; its ability to do so was of course guaranteed by its method of construction. So did the coefficient of contingency, at least for sufficiently large tables. Yule's coefficients, on the other hand, all failed abysmally. Not only were they on the whole poor approximations to the coefficient of correlation, but the values they took depended on where the arbitrary division between 'tall' and 'short' and 'heavy' and 'light' were taken. (7)

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- (7) For examples of this process of evaluation see Pearson (1900b, 15-18) and Pearson and Heron (1913, 193-202). Pearson's use of it can be found from the very beginning of his work on association. Thus, on 6 May 1899, before the appearance of the first published papers on the topic, he wrote to Yule pointing out to him that Q failed this test (Pearson Papers, C1 D6).

Given the basic interest in maximising the nominal/interval analogy, Pearson's use of the bivariate normal model makes sense. It was not that he was obsessed by the normal distribution. Quite the opposite: he was one of the first statisticians to point to the non-normal nature of many empirical distributions (Pearson, 1895), and had sought, albeit unsuccessfully, to develop a theory of correlation for non-normal variables which would fully take into account their non-normality.⁽⁸⁾ Pearson's position was pragmatic. If correlation is taken, as Pearson took it, to depend upon the specification of the function which best predicts the value of one variable from that of another, then something about the joint distribution of the two variables must be assumed. Only one joint distribution was, Pearson felt, sufficiently well known for this kind of analysis to be possible: the bivariate normal. Experience with the normal distribution had, he argued, shown that even if the assumption of normality was not strictly correct, inferences based on that assumption were unlikely to be seriously mistaken (Pearson and Heron, 1913). Thus, if one had to use a model, Pearson felt that the bivariate normal was best. Further/...

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- (8) Pearson felt that an approach to the correlation of non-normal variables must be built on knowledge of the particular form of their joint distribution, for only if this was known would it be possible to know how best to predict values of one variable from that of the other (Pearson, 1896, 274; Pearson, 1920b). Yule, by comparison, claimed that the ordinary product-moment coefficient could be used for these non-normal variables as it had an interpretation as the slope of the best-fitting line (in the least-squares sense) through their joint distribution, irrespective of the particular form of this distribution (Yule, 1897a). This difference of opinion can be seen in the letters of 1896 between Pearson and Yule in the Pearson Papers (C1 D6).

Further, some model was necessary if the nominal/interval analogy was to have any validity. For consider Yule's Q as an example of a coefficient not based on an explicit model. Values of Q are not comparable with those of the coefficient of correlation. Nor can comparability of the nominal and interval cases be achieved by reducing the interval data to two-by-two tables and applying Q , for the value of Q depends on the process by which this is done. Indeed, comparison of the values of Q from one two-by-two nominal table to another becomes, on this perspective, a process which is very difficult to justify. Without some model of the situation to give a meaning to coefficients of association, their comparative use appeared to Pearson dangerously arbitrary.

Pearson's approach to the theory of association was thus fairly tightly structured by the analogy between the association of nominal variables and correlation employed as a tool for interval-level prediction. Yule's approach was much looser. A coefficient of association in the nominal case (or, indeed, a coefficient of correlation in the interval case) was for him a measure of statistical dependence that need satisfy only general formal criteria (be zero for independence, one for complete dependence, and so on). Just to know that two variables are associated (that vaccination and survival, for example, are not independent) is obviously of some use in solving problems of prediction and control. Yule was not primarily concerned to be able to draw tighter inferences/...

inferences than this. Specific problems of prediction and control in specific contexts of use did enter into Yule's choice of particular coefficients (for example, between Q , w and r_{PS} in any particular instance) but did not structure Yule's overall formulation of the problem of association. (9) Yule can thus be seen as putting forward a general, formal theory of association which left a great deal of room for elaboration in specific instances. He did not seek a single best measure of association. Just as there are different measures of central tendency (mean, median, mode, and so on), there were, Yule felt, different ways of measuring association, which would yield different values for the same table. The superiority of one to the other could not be guaranteed in advance of the consideration of/...

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- (9) Indeed Yule was to come to doubt whether a coefficient of association was always what was needed. He wrote to Major Greenwood on 2 March 1915 (Yule-Greenwood Letters):

Here are the cholera arithmetic and diagrams. I have also enclosed a couple of sheets of lucubration on the measure of the advantage, and efficiency or effectiveness, of immunisation or similar processes. I cannot see my way to a measure of association, for I cannot get clear in my mind to begin with what we want to measure by the association coefficient: I seem to get more muddle headed whenever I try to think it out. In fact I don't seem really to want a measure of association at all. The 'advantage' or 'effectiveness' give what I want and neither is of the nature of an association coefficient, but the first is a regression and the second GOD knows what.

of particular applications. Attempts to do so on the basis of contentious assumptions (such as that of underlying distributions) were, Yule felt, simply dangerous and misleading. Yule felt that when working with nominal data one had to accept the limitation implied by the level of measurement: one was dealing with cases classed into categories, and nothing more. The statistician had to accept the data as given. Yule's methods were thus structured by a cognitive interest in prediction using nominal data as phenomena in their own right; the nominal/interval analogy had for him no direct force.

The differing cognitive interests of Pearson and Yule led to their two positions being incommensurable.⁽¹⁰⁾ Logic and mathematical demonstration alone were insufficient to decide between the two positions. Their concepts of 'measuring association' were different: for Pearson, it meant seeking to estimate an underlying correlation; for Yule, seeking in a looser sense to measure the dependence of the given nominal data. The same mathematical result would be interpreted differently by the two sides, in the light of their different cognitive interests.

Thus, both sides knew that for any given table Yule's/...

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(10) For the concept of incommensurability, see Feyerabend (1962) and Kuhn (1970, especially 148-50). The relationship of incommensurability to cognitive interests is discussed in Barnes and MacKenzie (1978).

Yule's three coefficients, Q , r_{PS} and w , would normally not agree, and sometimes would differ wildly in their values. For Pearson, this was sufficient to damn Yule's system utterly, for how could there be three different values for the association of one table? For Yule, on the other hand, this was fully to be expected, for Q , r_{PS} and w were simply different ways of summing up the observed data. Similarly, both sides accepted that the value of the coefficient of contingency was affected by the size of the table to which it was applied. For Yule, this was a severe weakness of the coefficient of contingency. Under certain circumstances its value reflected the number of cells in the table as much as the association of the data. For Pearson, on the other hand, this property was only to be expected. The coefficient of contingency was equal to the coefficient of correlation only in the limit case where the number of cells in the table became infinite. Therefore it was not surprising that the value of the coefficient of contingency should be affected by table size: on the assumption of an underlying normal distribution this could be corrected for. To take another instance, it was not disputed by either side that when applied to genuinely continuous binormal data, the value of Yule's Q differed considerably according to where the division (for example, between tall and short) was taken. For Pearson this invalidated Q . For Yule any property that Q had when artificially applied to interval data did not affect its use for nominal data, because he rejected/...

rejected Pearson's basic model of an underlying distribution.⁽¹¹⁾

7.5 Cognitive Interests and Goal Orientations

The differing cognitive interests manifested in the work of Pearson and of Yule were not accidental. They can be related to differing objectives in the development of statistical theory, and perhaps ultimately to differing social interests.

As has been shown in chapter four, Pearson's commitment to eugenics played a vital role in motivating his work in statistical theory. Pearson's eugenically-oriented research programme was one in which the theories of regression, correlation and association played an important part. The connection between these theories and eugenics had been first forged by their founder, Francis Galton, as was shown in chapter two. Pearson's work in statistical theory continued this link between the mathematics of regression and correlation and the eugenic problem of the hereditary relationship of successive generations.

In his first fully general discussion of the statistical approach to the theory of evolution, Pearson defined 'heredity' as follows (1896, 259):

Given/...

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(11) See Yule (1912, especially 145-6 and 159-63); Pearson and Heron (1913, especially 171-83 and 193-202); Pearson (1904b, 8-9); Pearson (1913a).

Given any organ in a parent and the same or any other organ in its offspring, the mathematical measure of heredity is the correlation of these organs for pairs of parents and offspring ... The word organ here must be taken to include any characteristic which can be quantitatively measured.

Two pages earlier Pearson (1896, 256-7) had explained that the correlation of two variables (he used the term 'organs') was what defined the function allowing the prediction of the value of one from that of the other. Put together, these notions of heredity and of correlation indicate what Pearson was doing. He was constructing a mathematical theory of descent, in order to be able to predict from the knowledge of an individual's ancestry the characteristics of that individual. Galton had solved the problem for the individual's parentage; Pearson wished to go further back and consider grandparents, great-grandparents, and so on.

Pearson's paper reveals two aspects of his attitude to correlation and its measurement. His notion of correlation, as a function allowing direct prediction from one variable to another, is shown to have its roots in the task that correlation was supposed to perform in evolutionary and eugenic prediction. It was not adequate simply to know that offspring characteristics were dependent on ancestral characteristics: this dependence had to be measured in such a way as to allow the prediction of the effects of natural selection, or of conscious intervention in reproduction. To move in the direction indicated here, from prediction to potential control over evolutionary processes/...

processes, required powerful and accurate predictive tools: mere statements of dependence would be inadequate.

Secondly, the prominence of correlation in his statistical thought can be seen to be related to the role of correlation as measuring the 'strength of heredity'. To define heredity as the correlation of parents and offspring indicates the a priori nature of Pearson's hereditarianism; that the correlation could be due to the similarity of parental and offspring environments was not even considered in this paper.⁽¹²⁾ It also indicates the possibility that the direct linking of correlation and heredity could well be the motor behind Pearson's work on the theory of correlation. If the study of heredity was to be increased in its scope, the theory of correlation had to undergo parallel development. In this paper of 1896, the move from consideration of parentage to entire ancestry was clearly associated with the development of the theory of correlation from Galton's two variable case to an indefinite number of variables.

The major restriction on Pearson's studies of heredity in the late 1890's was their limitation to measurable characteristics. Many characteristics, such as the colouration/...

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- (12) Later Pearson attempted to demonstrate the small role of environment by comparing 'coefficients of heredity' with correlations between the characteristics of children and particular aspects of their home environment; however, this was for him a subsidiary problem, as he believed that home environment was in any case largely a reflection of the innate characteristics of a child's parents. See, for example, Pearson (1910b).

colouration of animals and plants and the eugenically crucial mental characteristics of man, were not immediately susceptible to quantification (this period of course predates the invention of the Binet-Simon scale of 'intelligence'). All that was possible for these characteristics was classification of individuals into categories, and as the resulting data could not be analysed by an interval-level theory of correlation, there was no direct way of estimating the 'strength of heredity' for these characteristics. To extend research in heredity from interval to nominal characteristics required, given Pearson's operational definition of heredity, the extension of the theory of correlation from interval to nominal variables.

That this is the correct interpretation of the origins of Pearson's work on the theory of association is suggested by Pearson's own description of his problem situation:

Many characters are such that it is very difficult if not impossible to form either a discrete or a continuous numerical scale of their intensity. Such, for example, are skin, coat, or eye-colour in animals, or colour in flowers ... Now these characters are some of those which are commonest, and of which it is generally possible for the eye at once to form an appreciation. A horse-breeder will classify a horse as brown, bay or chestnut; a mother classify her child's eyes as blue, grey or brown without hesitation and within certain broad limits correctly. It is clear that if the theory of correlation can be extended so as to readily apply to such cases, we shall have much widened the field within which we can make numerical investigations into the intensity of heredity, as well as much lessened the labour of collecting data and forming records. (Pearson and Lee, 1900, 324-5)

Pearson's/...

Pearson's research on heredity did not simply provide the motivation for the development of his theory of association. It also conditioned the nature of that theory. In his problem situation can be seen the connection between his social Darwinian and eugenic goals and the cognitive interests manifest in his work on association. Pearson already had what he felt to be a satisfactory means for the investigation of the inheritance of interval characteristics, by the use of which he had accumulated a considerable body of 'coefficients of heredity'. In order to maximise the value of information on the inheritance of nominal characteristics, it was necessary to devise a 'coefficient of heredity' for them that paralleled that for interval characteristics. Therefore, the direction of development of the theory of association was, in the case of Pearson, determined by the need to maximise the analogy between the association of nominal variables and the correlation of interval variables. Pearson wanted to be able to say 'the coefficient of heredity for human mental ability is r ', and to compare that with the already calculated 'coefficients of heredity' for height, and other similar characteristics. A coefficient of association such as Yule's Q would not have enabled him to do this. As explained above, values of Q cannot be compared with that of the coefficient of correlation; nor can both height and mental ability data both be analysed by the use of Q , because of Q 's dependence on the arbitrary boundary between 'tall' / ...

'tall' and 'short'. For interval/nominal comparison to be plausible, Pearson needed a coefficient which, when applied to dichotomised height data, would yield a value as close as possible to that of the coefficient of correlation: hence Pearson's construction of r_T , and hence also his fundamental criterion of evaluation of coefficients of association.

Pearson had in fact begun collecting a set of primarily nominal data of great relevance to eugenics even before he had devised, in r_T , the necessary means of analysing it. Parent-child correlations were difficult to collect; Pearson however reasoned that the correlation of siblings (a term he introduced for pairs of brothers or sisters irrespective of sex⁽¹³⁾) were of equal theoretical value as measures of the strength of heredity.⁽¹⁴⁾ By circulating teachers, he obtained information on nearly 4000 pairs of siblings, including interval physical characteristics such as the cephalic index, nominal physical characteristics such as eye-colour, and a range of nominal mental characteristics such as 'ability' and 'conscientiousness'. The study was begun in 1898; by 1903 Pearson felt able to give a comprehensive survey of the results obtained in his Huxley Lecture/...

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(13) See K. Pearson (1914-30, 3A, 332).

(14) Sibling correlations and parent-child correlations were of course connected by the Galton/Pearson 'Law of Ancestral Heredity'; see K. Pearson (1898, 404-7).

Lecture to the Anthropological Institute (Pearson, 1903c; some early results were presented in Pearson, 1901c). This was Pearson's major contribution to the hereditarian theory of mental characteristics, and the forerunner of many later more sophisticated attempts to prove the dominance of nature over nurture.⁽¹⁵⁾ It is also his most central attempt to use r_T , and the one which most strongly drew Yule's criticism.

Pearson's analysis of mental ability can be taken as an example of his procedure. He had asked teachers to classify each of a pair of siblings into one of the following classes: quick intelligent, intelligent, slow intelligent, slow, slow dull, very dull and inaccurate-erratic. 'Very dull', for example, was defined as 'capable of holding in their minds only the simplest facts, and incapable of perceiving or reasoning about the relationship between facts' (K. Pearson, 1903c, 209). To permit the use of r_T , these seven categories were reduced to two, 'quick intelligent' and 'intelligent' forming one category, and the rest the other. Two-by-two tables were then drawn up, such as the following/...

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- (15) Three crucial differences between Pearson's work and later studies are the introduction of a numerical scale of 'intelligence', the use of twins as well as siblings in general, and the application of multifactorial Mendelian models (in addition to simple measures of resemblance) to gain estimates of 'heritability'. Important though these differences are, this later work can be seen as elaborating Pearson's basic approach, rather than diverging radically from it.

For an interesting point of view on Pearson's Huxley Lecture, see Welch (1970); see also the comments by E.S. Pearson (1972).

following for pairs of brothers, which is reconstructed from Pearson's data (1903c, 236):

| Second Brother | First Brother | | |
|---------------------------------------|---------------------------------------|-------|--------|
| | 'Intelligent' and 'Quick Intelligent' | Other | Totals |
| 'Intelligent' and 'Quick Intelligent' | 526 | 324 | 850 |
| Other | 324 | 694 | 1018 |
| Totals | 850 | 1018 | 1868 |

From these tables, values of r_T were then calculated (in this case $r_T = 0.46$).

Pearson found from these data measures of the 'strength of inheritance' for nine mental and nine physical characteristics, and was also able to bring into the comparison other previously produced estimates of the correlation of physical characteristics in pairs of siblings. Central to his argument were two assumptions, only partly explicit: the comparability of the coefficients of correlation for interval data and the value or r_T for nominal data; and the interpretation of these coefficients as measures of the 'strength of heredity'. On the basis of these assumptions, he was able to claim a remarkable finding: the strength of inheritance for a wide range of human mental and physical characteristics was virtually identical at around/...

around 0.5. Further, he claimed that environment played no significant part, and thus presumably assumed that residual effects (the fact that the correlation was only 0.5 and not 1.0) were simply the result of chance variations. Environment could, Pearson felt, be discounted because his series of characteristics included eye-colour. It was accepted that environment played no part in determining eye-colour, and yet the strength of inheritance for eye-colour was very close to the common 0.5. If environment played no part in the case of eye-colour, Pearson deduced that it therefore played no part in the other cases. Pearson's conclusion (1903c, 204) was a strong affirmation of rigorous hereditarianism:

We are forced, I think literally forced, to the general conclusion that the physical and psychical characters in man are inherited within broad lines in the same manner, and with the same intensity ... We inherit our parents' tempers, our parents' conscientiousness, shyness and ability, even as we inherit their stature, forearm and span.

Pearson thus had, by use of the tetrachoric coefficient, been able to forge a connection between physical and mental human characteristics along which inductive inferences could pass. It was, he felt, widely admitted that human physical characteristics were largely determined by heredity. By use of this channel of inference an identical conclusion could be drawn for mental characteristics. The polemical possibilities that this opened up for eugenicists were obviously important. Pearson was able to further extend them by bringing coefficients of heredity for various characteristics/...

characteristics in animals into the argument. In other species 'the resemblance of parent and offspring is again roughly .5' (Pearson, 1903c, 204). Thus, the generally accepted conclusion that in animal species 'good stock breeds good stock' (Pearson, 1903c, 206) could be extended to man.

At the end of the Huxley Lecture Pearson drew out the political conclusions which followed from his analysis. He talked of Britain's failure in imperialist competition with Germany and the United States, and the lack of intelligence and leadership that was the cause of it. His work, he argued, showed that the only solution was 'to alter the relative fertility of the good and the bad stocks in the community'.

That remedy lies first in getting the intellectual section of our nation to realise that intelligence can be aided and be trained, but no training or education can create it. You must breed it, that is the broad result for statecraft which flows from the equality in inheritance of the psychical and the physical characters in man.
(K. Pearson, 1903c, 207)

Given the contemporary concern for 'national efficiency', these were words in season, and were not without impact outside the scientific community. Pearson's lecture was quoted at some length by the Inter-Departmental Committee on Physical Deterioration (1904, 38-9), which had been set up by the Conservative Government as a result of the scare following early defeats of the British by the Boers in the South African War. Few of Pearson's contemporaries would have/...

have fully understood the mathematics of the tetrachoric coefficient, and few seem to have subjected his argument to close scrutiny, but the conclusion he was able to draw struck home.

Yule, on the other hand, had no commitment to eugenics. There is no record of his ever having made a public statement of his attitude to eugenics, nor do his letters to Karl Pearson, for example, reveal his opinions. Nevertheless, in his correspondence with the man who was perhaps his closest friend, Major Greenwood, it is possible to discover evidence of Yule's private views. These appear to have been a mixture of indifference and hostility, as the following quotations⁽¹⁶⁾ indicate:

... votes for women is to me nearly as loath-worthy [sic] as eugenics.

The Eugenics Congress is rather a joke ...

I've just got the letter from the Eugenics Ed[ucatio]n Soc[iety] asking me to lecture. I do not altogether like it ...

I am not a eugenicist, and I am not in the least keenly interested in eugenics.

When Yule's academic work touched on subjects of eugenic importance, a certain distance from the standard eugenic positions is apparent. On the issue of heredity versus environment he was cautious:

To/...

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(16) From the letters of Yule to Greenwood of 3 April 1912, 8 August 1912, 8 November 1912 and 17 August 1920 (Yule-Greenwood Letters).

To take an example from the inheritance of disease, the chances of an individual dying of phthisis depends not only on the phthisical character of his ancestry, but also very largely on his habits, nurture and occupation. (Yule, 1902, 228)

A major topic of Yule's early statistical work was pauperism, which the eugenists claimed to be a symptom of hereditary degeneracy. Yule, however, eschewed such arguments, and concentrated on the way administrative reforms, notably the abolition of out-relief, reduced the observed rate of pauperism (see Yule 1895-6; 1896; 1899).⁽¹⁷⁾

Even while he was a student of Pearson, Yule gave signs that he was to develop in an independent direction from his teacher.⁽¹⁸⁾ In 1893, aged 22, he became Pearson's demonstrator, assisting in the teaching of mathematics to engineering students and forming, along with Alice Lee, the audience for Pearson's first advanced course in mathematical statistics. In 1895 he was elected to, and became an active member of, the Royal Statistical Society. The concerns of this august but rather conservative body, rather than Pearson's social Darwinism, form the context of application for much of Yule's statistical work. While Yule's work was technically/...

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(17) It is not clear to me whether or not Yule was at any point motivated by a desire to oppose eugenics (for example in his attacks on Pearson's Huxley Lecture). All that is crucial to the argument on cognitive interests in relation to goal orientation is that Yule obviously lacked any positive commitment to eugenics.

(18) Biographical details for Yule are to be found in F. Yates (1952) and M.G. Kendall (1952).

technically far in advance of what the Royal Statistical Society was accustomed to, in subject, style and, indeed, in political assumptions, it would have been familiar. Thus, the Fellows were accustomed to an ameliorative orientation towards pauperism, and to Yule's focus on administration rather than the economy or social structure, even if the technical apparatus Yule employed was new.

It is possible that Yule may have come to realise the need for a measure of association while studying another favourite topic of the Royal Statistical Society, vaccination statistics. In 1897, during a discussion at the Society of an anti-vaccinationist paper, he made a long and highly critical comment on the author's use of statistical technique (Yule, 1897b). Consideration of the frequently dubious use of statistics in the vaccination debates then raging⁽¹⁹⁾ might well have prompted him to seek a standardised measure of the association between vaccination and survival during an epidemic. Cognitive interests associated with an ameliorative orientation to vaccination statistics may have played some role in structuring Yule's work on association.⁽²⁰⁾

(19) For these debates see MacLeod (1967b).

(20) In measuring the association of vaccination and survival it is obviously desirable for comparative purposes to have a measure which is independent of both the virulence of the epidemic (of the overall proportion of cases falling into the 'survived' and 'died' columns) and of the degree of activity of the medical authorities (proportions vaccinated and unvaccinated). Yule thus sought to construct coefficients which were unaltered by multiplication of any row or column by a constant. See Yule (1912, 113-23).

(21) See K. Pearson (1900, 42-5), Yule (1912) and Yule (1962a).

They did not, however, generate a search for a single measure of association as a unique property of the data. At most, the requirements of the vaccination question placed but loose constraints upon the evaluation of measures of association. For example, a shared convention was needed that would distinguish between intervention being totally without effect (no association) and intervention being totally effective (complete association). But no more general inductive inferences needed to be drawn. Yule's use of formal rather than substantive criteria in the construction of coefficients of association, his development of an empirical rather than a unitary theoretical approach, and his preference for dealing with nominal data as it was given, would all make sense in the light of this situation.

It was not, however, that Yule was developing a general theory of association while Pearson was developing one with only a limited sphere of application. Pearson strongly felt that his was a general theory, and applied it even to Yule's favourite cases such as vaccination statistics; Yule most strongly criticised the application of Pearson's theory to inheritance data.⁽²¹⁾ Both sides felt the theory of the other was wrong, and not merely misapplied. It was, rather, that Pearson's specific goal orientation led to a sophisticated and elaborate theory embodying specific cognitive interests, while Yule's more diffuse goal orientation led/...

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(21) See K. Pearson (1900b, 43-5), Yule (1912) and Yule (1906a).

led to a looser and more empirical approach which embodied cognitive interests of a more general nature.

7.6 Further Aspects of the Controversy

Up to this point I have treated the controversy as if it were simply a dispute between two individuals, Pearson and Yule. While these two were overwhelmingly the most active participants, it is important to look at the involvement of others in the British statistical community. The group of scientists contributing to the development of statistical theory in Britain in the period 1900 to 1914 was small. A list produced using Kendall and Doig's Bibliography of Statistical Literature (1968) consists of 26 individuals who can be seen as having in some sense an active ongoing interest in the development of statistical theory.⁽²²⁾ Of these, twelve can be regarded as members of Pearson's biometric school, since they had close institutional or personal ties to the Biometric and Eugenic Laboratories at University College, London, and their preferred medium for publication seems/...

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(22) The list omits those who wrote only one paper in the field and who did not, therefore, seem to have had an ongoing active interest in it. The most obvious problem of inclusion/exclusion is the decision as to whether a piece of work contains a development of statistical theory and method or simply an application of existing methods. Thus, for example, Charles Spearman is included but Cyril Burt excluded, and while this does indicate real differences in the type of work they did, it shows that there is no absolute division between those included and those excluded.

J.M. Keynes, G.J. Lillie, A. A. Kendrick, V.F. Sheppard, C. Spearman, G.H. Thomson, G.V. Yule.

seems to have been Biometrika. The other 14 had a wide variety of affiliations, and included civil servants, administrators and one industrial scientist, as well as university staff. (23)

Ten of the twelve biometric school members either took part in attacks on Yule on this topic (Pearson, Heron), contributed to the theoretical discussion or development of the Pearsonian approach (J. Blakeman, W.P. Elderton, Everitt, Heron, Pearson, Snow, Soper) or used the tetrachoric coefficient in empirical work (E.M. Elderton, A. Lee, E.H.J. Schuster and all above except Blakeman and Soper). In the remaining two cases (Galton and Isserlis), I have not been able to find evidence of attitudes. Galton died in 1911, before the controversy came to a head; the work of Isserlis on the theory of statistics was just beginning at the end of this period.

This overall pattern is as one would expect. The tetrachoric method and the related later developments were part of the distinctive approach of the biometric school, were widely applied to empirical data, primarily in the eugenic field, and were the focus of theoretical attention/...

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(23) I would class the following as members of the biometric school: J. Blakeman, E.M. Elderton, W.P. Elderton, P.F. Everitt, F. Galton, D. Heron, L. Isserlis, A. Lee, K. Pearson, E.H.J. Schuster, E.C. Snow, H.E. Soper. The 'others' are A.L. Bowley, J. Brownlee, F.Y. Edgeworth, R.A. Fisher, W.S. Gosset, M. Greenwood, R.H. Hooker, J.M. Keynes, G.J. Lidstone, A.G. McKendrick, W.F. Sheppard, C. Spearman, G.H. Thomson, G.U. Yule.

attention. As was described in chapter five, the biometric school was a tightly-knit, coherent group, a large part of whose funding came from its activities in eugenic research.⁽²⁴⁾ This research was a team activity in which data collection, the development of the necessary mathematical theory, computation, and so on, were closely integrated under/...

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- (24) The following skit, written by Major Greenwood (Greenwood to Yule, 8 November 1913; Yule Papers, box 1), indicates the extent to which the opponents of Pearson's methods felt themselves up against a coherent group with a clear orthodoxy:

Extracts from The Times, 1 April 1925

G. Udny Yule, who had been convicted of high treason on the 7th ult. was executed this morning on a scaffold outside Gower St. Station. A short but painful scene occurred on the scaffold. As the rope was being adjusted, the criminal made some observation, imperfectly heard in the press enclosure, the only audible words being 'the normal coefficient is - '. Yule was immediately seized by the Imperial guard and gagged. The coroner's jury subsequently received evidence that death had been instantaneous. Snow was the executioner and among others present were the Sheriff, Viscount Heron of Borkham and the Hon. W. Palin Elderton.

Up to the time of going to press the warrant for the apprehension of Greenwood had not been executed, but the police have what they regard to be an important clue. During the usual morning service at St. Paul's Cathedral, which was well attended, the carlovingian creed was, in accordance with an imperial rescript chanted by the choir. When the solemn words, 'I believe in one holy and absolute coefficient of four-fold correlation' were uttered a shabbily dressed man near the North door shouted 'balls'. Amid a scene of indescribable excitement, the vergers armed with several volumes of Biometrika made their way to the spot, but one of them was savagely bitten in the calf by a small mongrel and in the confusion the criminal escaped.

under the personal supervision of Karl Pearson. So a relationship between the needs of eugenic research and the cognitive interests manifested in the development of the theory of association by the biometric school can reasonably be held to exist, irrespective of the particular motives of individual members of the school. I have not been able to discover whether P.F. Everitt, say, who drew up the tables of tetrachoric functions to permit easier calculation of r_T , shared Pearson's beliefs. The point is, however, that he was working to overcome a difficulty which had arisen within the context of an integrated research programme in which the demands of eugenic research generated, and conditioned the solution of, particular technical problems.

One important British statistician can be seen as leaving the biometric school in this period: Major Greenwood (1880-1949). In his case, as was described in chapter five, two parallel processes can be observed in the period 1910 to 1914. He left the immediate group of researchers round Karl Pearson at University College to take up a post of statistician at the Lister Institute of Preventive Medicine. At the same time, perhaps as a result of his move into a new academic field which traditionally stressed environmental causes of disease, he became critical of eugenic doctrines. His attitude to the measurement of association also underwent a change in this period. In 1909, in a paper arguing the importance of the hereditary factor in tuberculosis, Greenwood/...

Greenwood used the tetrachoric coefficient, describing it as the 'exact' and 'true' method of measuring association (1909, 259). Soon, however, he became first a private and then a public critic of the tetrachoric method.⁽²⁵⁾ While the available evidence does not permit identification of a causal sequence linking these different changes, the case of Greenwood adds weight to the association between membership of the biometric school, scientific work in the eugenic field, and use of the tetrachoric and other Pearsonian methods.

What of those statisticians who were not members of the biometric school? Of these only one, John Brownlee, seems to have been an enthusiast for the tetrachoric method. He was a member of the Glasgow Branch of the Eugenics Education Society.⁽²⁶⁾ Yule, Greenwood and Brownlee apart, only two 'non-biometric' statisticians seem to have publicly committed themselves on the measurement of association: F.Y. Edgeworth and R.H. Hooker. Neither, as far as I am able to tell, was a eugenicist.⁽²⁷⁾ Both were members of the/...

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(25) See his letters to Yule in this period in the Yule Papers (box 1) and Greenwood and Yule (1915).

(26) See Eugenics Education Society (1911; 1912). Brownlee even used r_T in the case of theoretical Mendelism, where the biometricians denied its applicability: see Brownlee (1910) and Snow (1912).

(27) Edgeworth's one flirtation with hereditarianism is described in chapter five. I have not been able to find any writings by Hooker dealing with eugenics.

the Royal Statistical Society, and it was at a meeting of it that they gave at least qualified support to Yule (Edgeworth, 1912; Hooker, 1912); the Society seems, in fact, to have been the closest Yule came to having an 'institutional base'. Clearly it was in no way comparable to Pearson's Biometric and Eugenic Laboratories, with their own publications and journal, but at least the Society provided Yule with a sympathetic hearing and a place to publish his major attack on Pearson as well as other more minor writings on association.

Thus, consideration of British statisticians other than Pearson and Yule seems to confirm in broad terms the association of Pearson's approach with the needs of eugenic research and that of Yule with the broader and less specific needs of general applied statistics. However, before moving to the final stage of the argument, it is necessary to consider other possible explanations of the controversy, and to examine briefly the history of the measurement of association after 1914.

It might be argued that Pearson's philosophical views account for his attitude to the measurement of association. However, it would seem that his approach, with its use of hypothetical underlying variables, violates rather than exemplifies the positivist and phenomenalist programme of The Grammar of Science (K. Pearson, 1892a). The practical demands of his research proved stronger than his/...

his formal philosophy of science. His characterisation (Pearson and Heron, 1913, 302) of the dispute as between his 'nominalism' and Yule's 'realism' can indeed be turned on its head. In their concepts of correlation Pearson was the 'realist' and Yule the 'nominalist'. Pearson's Huxley Lecture argument, for example, rests on the interpretation of a correlation as the measure of a real entity, as a strength of heredity, and largely collapses if a correlation is seen as merely the name for an observed pattern of data. Pearson's general cosmological bent towards continuity and variation rather than homogeneity and discrete entities (discussed in section 6.5) may in part account for his rejection of methods such as r_{PS} (which involved treating individuals in a given category as in a certain sense identical), but cannot, it seems to me, account for the specific features of Pearson's methods of measuring association.

Psychological explanations (such as a clash of personalities) also seem inadequate. Personal relations between Pearson and Yule seem to have been soured as a result of disagreement, rather than disagreement being caused by personal antagonism.⁽²⁸⁾ The divergence of views was already/...

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(27) This was the account given by Yule to Greenwood in his letters of 18 May and 26 May 1936 (Yule Papers, box 2). The Yule-Pearson correspondence (Pearson Papers, C1 D3 and C1 D6) bears this out, as it continues on an amicable basis up to Yule's first criticism of Pearson in 1905 and is then abruptly terminated (apart from three letters of 1910, dealing with a personal matter).

already present in the perfectly amicable papers of 1900. Even if Pearson and Yule had remained the best of friends they would still have measured association differently, and this difference would still have to be explained.

A third possible alternative explanation might be that non-eugenic biometrical concerns were of equal or greater importance in leading to Pearson's development of the tetrachoric method. It is certainly true that Pearson used r_T to measure the 'strength of inheritance' in organisms other than man. But to separate a 'neutral' biometry from an 'ideological' eugenics would be ahistorical and would fail to capture the integral nature of Pearson's thought. The results of the biometric studies of heredity in animals were used in Pearson's eugenic argument: the channel of inference from the animal world to human physical characteristics to human mental characteristics was crucial to Pearson's position.

How did the controversy end? Debate virtually ceased at the time of the First World War. Two factors may have been involved in this. After 1918 the huge amount of data on inheritance of human and animal characteristics flowing into the Biometric and Eugenic Laboratories was much reduced. 'The post-war years were not favourable to the spread of Galton's eugenic creed' and in Pearson's work 'eugenics was for the moment set aside' (E.S. Pearson, 1936-8, part 2, 205, 206). Thus, the immediate importance of the problem/...

problem for Pearson was reduced, and much less theoretical and practical work on the measurement of association was done at the Biometric and Eugenic Laboratories. Secondly, a new approach to eugenics and statistics was developing, most notably in the work of R.A. Fisher, which focused attention on different problems. Fisher used Mendelism in order to measure the 'strength of heredity' in a way that was different from that of Pearson. He employed an analysis of variance scheme based on a theoretical Mendelian model, rather than the direct comparison of correlation coefficients (Fisher, 1918a; see section 6.6 and Norton, 1977). While Fisher did not reject Pearson's work on the inheritance of mental characteristics, his own research programme led him beyond it in a way that did not require the use of coefficients of association.

The controversy was not, however, resolved. Contemporary statistical opinion takes a pluralistic view of the measurement of association, denying that any one coefficient has unique validity. The influential work of Goodman and Kruskal (1954-9, part 1, 763) argued that measures 'should be carefully constructed in a manner appropriate to the problem in hand' in such a way as to have operational interpretations. The general approach of modern statisticians is thus closer to that of Yule than that of Pearson. Yule's Q remains a popular coefficient, especially amongst sociologists (see, for example, Davis, 1971). Pearson's tetrachoric coefficient, on the other hand/...

hand, has almost disappeared from use except in psychometric work (for example, Castellon, 1966). It is interesting to speculate whether this situation can be explained in terms of, on the one hand, the sharing by most modern statisticians of Yule's lack of an overall, specific goal-orientation and, on the other, the continuing influence of hereditarianism in psychometrics. This point could, however, be established only by an analysis of the contemporary literature, which is outside the scope of this study.

7.7 The Controversy and Social Interests

The preceding analysis has shown that Pearson's and Yule's theories of association were structured by different cognitive interests, and that these different interests can be accounted for in terms of the relationship between Pearson's statistical theory and his eugenics research, and the lack of any similar relationship in the case of Yule. Pearson drew his support almost exclusively from the tightly-knit group of researchers, under his leadership, which was pursuing a unified research programme in statistics, biometry and eugenics. Yule gathered what support he could from individuals who were not enthusiastic about eugenics, and whose chief organisational link appears to have been the Royal Statistical Society.

The final stage of the analysis is necessarily very tentative, and involves (as did the corresponding hypothesis in chapter six) a structural argument which cannot be/...

be proven from the material presented here. The structural argument is quite simple. It is that eugenics was sustained by the social interests of certain sectors of British society, and not by those of other sectors.

One part of this structural hypothesis has already been presented in chapter three: that eugenics reflected the social interests of a rising professional middle class. The other part of it was suggested in section 6.5 in the discussion of William Bateson's attitude to eugenics: that declining élites which had turned to conservatism in reaction against bourgeois progress would not find the British eugenics of this period a suitable expression of their social interests. These groups certainly would have no reason to defend the urban 'residuum', nor to call into question the hierarchical division between mental and manual labour, and certainly were unlikely to be attracted to assertions of the inherent equality of all. However, the scientific, interventionist, middle class nature of eugenics was not likely to attract them. While they might share many of the basic premises of hereditarian thought, British eugenics as a concrete cultural form would be alien to them.

Such general evidence as is available on the social distribution of attitudes to eugenics seems to support this hypothesis. That support for eugenics came almost exclusively from within the ranks of the professional middle class is documented in chapter three. Opposition to/...

to eugenics came from such diverse groups as right-wing conservatives, the Catholic Church and defenders of traditional civil liberties,⁽²⁸⁾ as well as from more predictable quarters such as revolutionary socialists and those professionals (such as public health workers) with commitments to particular environmental reforms. The types of argument used in support of or against eugenics are also compatible with the hypothesis. Eugenic writings were quite often explicit propaganda for the professional middle class and for the social value of their skills. Conversely, one of the main strands of opposition to eugenics was the defence of reproduction as an area of traditional religious authority (or of personal free choice) against the encroachments of science and expertise.

What of our two main participants? Pearson's work has been analysed in chapter four as an especially clear and important contribution to the development of eugenics as an ideology of the professional middle class. Yule is more problematic. Unlike Pearson, Yule was reticent about his social, political and philosophical attitudes, so there is a poverty of definite information on which to draw. What does, I think, emerge from his letters, from comments on him by those who knew him well, and from occasional passages in his writings, is a personally genial but at the same/...

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(28) See section 3.8 and Searle (1976, 110-11).

same time fundamentally detached, sceptical and conservative man. Major Greenwood wrote of Yule that 'politically, even in university politics, he is a stern, unbending Tory' (Greenwood to Pearl, 19 August 1926; Pearl Papers). In later life Yule turned to religion (Yule to Greenwood, 2 February 1936; Yule-Greenwood Letters). On several politically-relevant scientific issues, his position was radically different from that of Pearson. As against Pearson's orthodox Darwinism, Yule advocated the anti-Darwinian and mutationist views of J.C. Willis (Yule, 1924; see Willis, 1922). As against Pearson's 'entrepreneurial' and 'socially-relevant' science, Yule's ideal of the scientific researcher was of a 'loafer of the world', free from ties, grants and commitments (Yule, 1920). As against Pearson's positivism, Yule was suspicious of the cult of measurement (Yule, 1921, 106-7). In his social position, Yule can, at least in his early life, be seen as downwardly mobile. He came from an old-established élite family of army officers, Indian civil servants and orientologists. Both his father and his uncle had been knighted. The family's wealth does not, however, seem to have been transmitted to Yule. In the absence of a sufficiently well paying statistical job, he was forced, during most of the period discussed here, to take an administrative position in a board examining apprentice craftsmen and technicians and to lecture in the evenings to clerks. While Yule's social situation cannot be seen as predetermining his attitudes (there was nothing to stop him deciding, say, to throw in his hand with the eugenicists or Fabians rather than to/...

to remain aloof), his career and beliefs, taken as a whole, can perhaps be seen as instancing possible connections between a declining élite, general conservatism and distaste for eugenics.

In the light of the institutionalised nature of the connection between statistics and eugenics in the biometric school, there would be little point in examining the social situation of individual biometricians other than Pearson. Although it would appear that in fact those of Pearson's students for whom information is available can in general be seen as 'rising professionals' rather than 'members of a declining élite' (for example, David Heron, who came from a Scottish village school up through the education system to be a leading figure in government and academic circles (E.S. Pearson, 1970a)), this sort of information is not of central importance. Yule's supporters are of somewhat greater interest, in that Yule was not the head of a research institution nor in any position of power, and therefore we can be rather more certain that those who supported him did so out of conviction. Both Hooker and Edgeworth were similar to Yule in background. R.H. Hooker was the son of Sir Joseph Dalton Hooker and grandson of Sir William Hooker, both Directors of the Royal Gardens at Kew; he himself had a humbler career as a civil servant in the Board of Agriculture (Yule, 1944). Francis Ysidro Edgeworth came from an old and distinguished family of Anglo-Irish gentry (Edgeworthstown, County Longford was their family/...

family seat), but one that was in particularly sharp decline. Although Edgeworth was the fifth son of a sixth son, he was the last in the male line of the Edgeworth's, and by the time he had inherited it the family estate had sunk into neglect (Keynes, 1926; Bowley, 1934). On the other hand, Greenwood, although listed in Burke's Landed Gentry, can, as the son and grandson of doctors, be better placed in the body of the professional middle class. His case (in which it has been hypothesised that his later occupational position in public health led him away from his early eugenic commitment) indicates the complexities of the relationship between class position and eugenic belief.

Although too much weight should not be placed on this kind of evidence, it can be noted that the biographies of the individuals discussed here are broadly compatible with the pattern expected on the basis of the structural hypothesis.⁽²⁹⁾ On this hypothesis, it is claimed that eugenics was sustained by particular social interests. To the extent that this was so, and to the extent that the preceding sections have shown the importance in the controversy of the goals set by eugenic research, it can be concluded that social interests entered through the 'mediation' of eugenics into this episode in the development of statistical theory in Britain.

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(29) In the light of the biographical information on Yule, Edgeworth, Hooker and Bateson, it is interesting that Levitas (1976, 547) suggests that Christian Socialism - a paternalist, anti-bourgeois movement - was largely composed of individuals downwardly mobile from the pre-industrial élite.

Chapter Eight

New Directions

In this chapter I shall examine two social and intellectual processes that, although only beginning in the period discussed here, became of increasing importance from the 1930's onwards. Firstly, a new occupational role developed: that of the statistician employed in industrial and agricultural research. Secondly, with the establishment of statistical theory as an ongoing activity with a network of communication, there developed concern for what might be called 'metastatistics': systematic reflection on the theory and practice of statistics. To give an adequate account of these developments, even in their earliest stages, would be far beyond what is possible here, and to describe fully the work in statistical theory of the main figure discussed here - R.A. Fisher - would in itself require a separate thesis. Instead, I shall focus on one issue only: the development of the theory of statistical inference. I shall attempt to sketch some initial effects on the theory of statistical inference of the above processes. This is intended simply as an epilogue to the account of British statistics given in the preceding chapters, and as a pointer to future developments, and does not claim to be a comprehensive or detailed study of this fascinating topic.

First/...

First, it will be necessary to examine briefly the 'traditional' - Bayesian - mode of statistical inference. The common habit of referring to the frequentist - non-Bayesian - mode of inference as 'classical' is, as this section will show, quite at variance with the actual development of the theory of inference: Bayesian inference was the historical predecessor of frequentist inference. In section 8.2 the approach to inference of Karl Pearson and the biometric school will be discussed. Pearson's continuing employment of Bayesian inference will be described, as will his reliance on large sample assumptions. In section 8.3 the work of W.S. Gosset ('Student') will be discussed. In his statistical theory Gosset broke decisively with the assumption of large sample size, and the reason why he did so will be presented. The following section deals with the early work of R.A. Fisher, who first developed a comprehensive non-Bayesian theory of statistical inference. The final section then discusses Fisher's period of work in the 1920's at the agricultural research station at Rothamsted, and points to the significance of Fisher's book, Statistical Methods for Research Workers (1925), in providing an exemplar of a new occupational role for the statistician.

8.1 The Traditional Approach to Inference

Much of statistical theory - especially twentieth century statistical theory - is concerned with the problem of inference. Put crudely (and here crudeness is an advantage/...

advantage in that precision is explicitly theory-laden) the problem is one of the nature of the statements that statisticians can make on the basis of their analyses. Typically, they will have data on only a subset of the cases they are interested in, and will wish to say something about the whole set. They may want to make a prediction, on the basis of past experience, as to what will happen in the future. They may wish to change their estimates of the plausibility of a hypothesis in the light of an experiment. They may wish to say something about a population on the basis of having examined a sample of it chosen at random. They may wish to phrase what they have to say not in the form of a statement, but in that of a decision or recommendation for action. Generally, they want to infer from the known and examined to the unknown and unexamined. In doing this, statisticians are no different from other scientists, or indeed from ordinary members of society. However, part of the expectations surrounding the role of statistician is that they should do this as reliably as possible, and should use the specialised knowledge of their discipline in order to do so.

In the contemporary world, problems of statistical inference tend to be closely linked with technical prediction and control: for example, in the techniques of quality control. However, the historical roots of statistical inference lie elsewhere. Much of the framework of inference was developed in the context of problems of belief in a general/...

general sense, and in particular of theological belief. The problem of inference was this: given our limited knowledge, ought we to believe in God? Or, given our lack of knowledge of God, what is the rational decision to take with regard to Christianity? The concept of probability was used to interpret and give meaning to decisions about religion. By metaphoric extension a concept from games of luck (that of chance, hasard) was linked to the old, non-quantitative concept of probability, used, for example, by the schoolmen of the Middle Ages to discuss particular doctrines of Christianity that were disputed (Hacking, 1975).

As Hacking (1972; 1975, 63-72) points out, this tradition stems initially from Pascal's famous 'wager' on the existence of God. Given wide circulation in the Jansenist Logic (Arnauld and Nicole, 1965; first published in 1662), the theological use of probability stimulated, inter alia, Jacques Bernoulli's famous discussion of the 'art of conjecturing' (Hacking, 1971a; Hacking, 1975, 143-53). In Britain, Newtonian natural theology provided much of the framework for the eighteenth century development of probability theory (K. Pearson, 1924, 404; P. Buck, 1977, 83-4). In the introduction to the most important early work of probability theory to be published in English, The Doctrine of Chances, de Moivre (1738, v) explained the theological relevance of the theory of probability:

... we may learn, in many Cases, how to distinguish the Events which are the effects of/...

of Chance, from those which are produced by Design: The very Doctrine that finds Chance where it really is, being able to prove by a gradual Increase of Probability, till it arrive at Demonstration, that where Uniformity, Order and Constancy reside, there also reside Choice and Design.

John Arbuthnot (1710) provided the best-known example of the kind of analysis de Moivre had in mind. He demonstrated that the persistent slight excess of male over female births observed in London was extremely unlikely to be the result of chance deviation from a true 1:1 ratio. God, he concluded, was intervening to ensure an extra supply of male babies to counterbalance the higher male infant mortality rate, thus making the holy sacrament of marriage available to the whole population.

The Reverend Thomas Bayes agreed that probability theory should be no trivial pastime. He wrote:

So far as Mathematics do not tend to make men more sober and rational thinkers, wiser and better men, they are only to be considered as an amusement, which ought not to take us off from serious business.
(Quoted by Barnard, 1958, 132)

The problem to which Bayes addressed himself was, in the words of his friend Richard Price, to 'give a clear account of the strength of analogical or inductive reasoning' (Bayes, 1764, 135; Price's emphasis). De Moivre and others had not, according to Price, fully achieved the main purpose of the doctrine of chances, namely:

... to shew what reason we have for believing that there are in the constitution of things fixt laws according to which events happen, and that, therefore, the frame of the world must be the effect of the wisdom and power of an/...

an intelligent cause; and thus to confirm the argument taken from final causes for the existence of the Deity.
(Bayes, 1764, 135)

The precise problem Bayes dealt with was this:

Given the number of times in which an unknown event has happened and failed: Required the chance that the probability of its happening in a single trial lies somewhere between any two degrees of probability that can be named.
(1764, 136; Bayes's emphasis)

Bayes assumed that the probability of the event happening was constant but unknown: call this unknown probability θ . He supposed that we start from a state of complete ignorance, so that all values of θ between 0 and 1 are equally likely. On this basis, he showed that if an event has been observed to happen p times, and to fail q times, the probability, a posteriori, of θ lying between a and b ($0 \leq a \leq b \leq 1$) was:

$$\frac{\int_a^b \theta^p (1-\theta)^q d\theta}{\int_0^1 \theta^p (1-\theta)^q d\theta}.$$

(This expression is that given by Todhunter (1865, 295).

Bayes tended to talk of areas under curves rather than integrals.)

The underlying idea in Bayes's approach was generalised by P.S. de Laplace (1814, especially 177-88 and 363-401), and became the foundation of nineteenth century theories of inference. British writers typically referred to it as the 'method of inverse probability'.⁽¹⁾ At the cost/...

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- (1) In 'direct' probability we work from a model or description of a situation to deduce consequences: this urn contains five white balls and five black balls, therefore the probability of drawing a white ball is 0.5. 'Inverse' probability arguments occur when, for example, we work backwards from observation of the outcome of drawing balls to reach probabilistic conclusions about the proportion of white and black balls in an urn.

cost of some anachronism, it can be presented in brief as follows. Let X be a random variable with a frequency distribution, $f(x|\theta)$, dependent on an unknown parameter θ . On the basis of previous knowledge and experience, we ascribe a prior probability distribution, $\pi(\theta)$, to θ . We then perform an experiment, or make certain observations, and obtain a sample of n independent values of X . Call these n values x_1, x_2, \dots, x_n , or collectively, \underline{x} . Let $g(\underline{x};\theta)$ be the probability of these values occurring for a given value of θ . Then,

$$g(\underline{x};\theta) = f(x_1|\theta) f(x_2|\theta) \dots f(x_n|\theta)$$

The posterior distribution of θ , $\pi'(\theta|\underline{x})$, is proportional to the prior distribution of θ multiplied by the probability of the observed sample for any given value of θ :

$$\pi'(\theta|\underline{x}) \propto g(\underline{x};\theta) \pi(\theta)$$

Now let us make what the nineteenth century writers called the assumption of the 'equal distribution of ignorance': let us assume that the prior distribution of θ is uniform, or, in other words, that $\pi(\theta)$ is a constant, and does not depend on θ .⁽²⁾

Then/...

- - -

- (2) For what happens if the range of possible values of θ is infinite, see D.V. Lindley (1965, 2, 18-19). Full consideration of this issue, which involves some difficult and contentious points, would lead us too far away from the historical material to be discussed.

Then $\pi'(\theta | \underline{x}) \propto g(\underline{x}; \theta)$

or $\pi'(\theta | \underline{x}) = C g(\underline{x}; \theta)$

where C is a constant, not involving θ , that can be evaluated by use of the condition that the integral of $\pi'(\theta | \underline{x})$ over the range of θ must be equal to unity.

The last equation for the posterior distribution of θ implies that - if the assumptions of this analysis are granted, and the constant C is ignored - we can treat $g(\underline{x}; \theta)$ (the 'likelihood function', to use an anachronism) as the posterior distribution of θ . This appears to have been the meaning to nineteenth century British mathematicians of the 'method of inverse probability' (for example, Todhunter, 1865, 584 and 592). The assumptions of the method - such as that of the 'equal distribution of ignorance' were by no means always explicit. Indeed, it seems not entirely unlikely that many mathematicians might have remembered the conclusion of the analysis - that a 'likelihood function' could be treated as a posterior distribution - without in all cases being fully conscious of the method by which it had been reached.

(In passing, it should be noted that the 'method of inverse probability' is indeed a generalisation of Bayes's original result. The latter can be derived from the former as follows. If the probability of an event happening is θ , then the probability of the observed sample - of the event happening p times and failing q times - is

$$\frac{(p+q)!}{p! q!} \theta^p (1-\theta)^q.$$

This/...

This, then, is $g(\underline{x};\theta)$. On the 'method of inverse probability' it (multiplied by the constant G) can be treated as the posterior distribution of θ . In other words,

$$\pi'(\theta|\underline{x}) = C \frac{(p+q)!}{p! q!} \theta^p (1-\theta)^q.$$

Now $\int_0^1 \pi'(\theta|\underline{x}) d\theta = 1$, as θ must lie in the range $0 \leq \theta \leq 1$. So

$$C = 1 / \left\{ \frac{(p+q)!}{p! q!} \int_0^1 \theta^p (1-\theta)^q d\theta \right\}.$$

Hence

$$\pi'(\theta|\underline{x}) = \frac{\theta^p (1-\theta)^q}{\int_0^1 \theta^p (1-\theta)^q d\theta}$$

and so the posterior probability of θ lying between a and b

($0 \leq a \leq b \leq 1$) is

$$\frac{\int_a^b \theta^p (1-\theta)^q d\theta}{\int_0^1 \theta^p (1-\theta)^q d\theta}.$$

This, of course, is Bayes's result.)

Many modern statisticians would find the 'method of inverse probability' unconvincing, in part because of doubts about the nature of the concept of probability underlying the method. It is interesting, however, that, for at least the early part of the nineteenth century, this was not a contentious issue. From the time of Pascal to the middle years of the nineteenth century, two approaches to probability that were later to be distinguished were not differentiated. These Hacking (1971b; 1975) calls the 'epistemological' and the 'physical' interpretations. In the epistemological interpretation, 'Prob($a|b$) expresses a relation between an hypothesis a and some evidence b ' (1971b, 340). This relation can be interpreted as a question of degree/...

degree of logical support, or, subjectively, as a question of personal belief. In the physical interpretation, on the other hand:

... Prob (a|b) expresses a physical feature of a chance set-up on which one might make repeated trials. It denotes the relative frequency with which outcomes of kind a would occur among outcomes of kind b.
(Hacking, 1971b, 340)

Physical probability in this sense can be talked of either in terms of a limiting ratio in an infinite sequence of trials, or in terms of a 'dispositional property'.

The key to much of the early vitality of probability theory was the metaphoric link formed between the epistemological interpretation (drawn from theology) and physical interpretation (drawn from games of chance). As Hacking points out, early writers tended to give a definition of probability in terms of equally possible cases. Thus P.S. de Laplace defined probability as 'le rapport du nombre des cases favorables à celui de tous les cas possibles' (1814, vii). Despite the manifest inadequacy and apparent circularity of this definition (can we define equally possible cases except in terms of probability?), it survived and flourished, because by using it writers 'could usefully equivocate' between epistemological and physical interpretations of probability (Hacking, 1971b, 341).

'Probability' then served as a 'gloss' in the ethnomethodological sense (Garfinkel and Sachs, 1970). Most probability theorists, if called upon to define 'probability'/....

'probability', would have given the 'equally possible cases' definition. But in doing so, they were merely retreating from one gloss to another. Mathematicians concentrated on 'doing' probability theory, and in this process the meaning of the central gloss developed.

P.S. de Laplace's great Théorie Analytique des Probabilités showed the richness of the structure that could be developed on this basis. Nevertheless, during the nineteenth century British writers began to criticise the foundations of probability theory, largely from an empiricist point of view, and sought to narrow and clarify the concept of probability.⁽³⁾

The most important of these works on the foundations of probability theory was John Venn's The Logic of Chance, first published in 1866.⁽⁴⁾ Venn's position was that in considering probability 'experience is our sole guide' (1866, 26). The proper objects for analysis by the theory of probability are, Venn argued, series of observed events, not gradations of belief. He defined the probability of an event in terms of the limiting value of its relative frequency in an infinite series of trials (1866, 107-8). The/...

(3) In passing, it is interesting to note that efforts to reach a satisfactory and universally-applicable definition of probability have proved futile, and that mathematicians have responded to this by considering 'probability' as an undefined term that satisfies a particular set of axioms, that is, is used in a certain way within mathematical 'talk'.

(4) For a general account of nineteenth century developments in this area, see Laudan (1973).

The Bayes/Laplace principle of the 'equal distribution of ignorance' was attacked by Venn as lacking justification in experience, and leading in certain cases to absurd results.

Venn's empiricist argument did not overturn the 'method of inverse probability'. Edgeworth, who had considerable interest in problems of inference, defended the traditional approach on philosophical grounds (Edgeworth, 1884), and continued to use Bayesian methods in practice (Edgeworth, 1908-9).⁽⁵⁾ When Karl Pearson first turned to problems of inference - in pursuit of his studies in the philosophy of science - he too remained within a basically Bayesian framework. Given his desire to base all knowledge on experience, subjective interpretations of probability were unattractive to him: he worked with at least a loosely frequentist view of probability. But 'inverse probability' arguments he found useful because of the way they could be employed to justify induction. In the Grammar of Science he adopted a compromise position: he sought to justify the 'principle of the equal distribution of ignorance' on empirical grounds (Pearson, 1892a, 174-5). Neither wholehearted 'degree of belief' Bayesianism nor a complete rejection of inverse probability methods were attractive/...

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(5) In this - his most important work on inference - Edgeworth did, however, attempt a 'direct probability' justification of his procedure. See Edgeworth (1908-9, addendum) and Pratt (1976).

attractive to him. As the next section will show, his practical statistical experience did not force him to abandon this compromise.

8.2 The Biometric School's Approach to Inference

In his views on inference Karl Pearson was, ultimately, a Bayesian. Certainly, there is evidence that he was not a particularly happy or confident Bayesian, and his doubts may have grown with the passage of time, but his work remained, broadly speaking, within the framework of the traditional approach to inference.

Pearson's Bayesianism is most strongly evidenced by the major early contribution of the biometric school to the theory of inference: Pearson and Filon (1898). Admittedly, the Bayesianism of this rather difficult paper is not explicit. E.S. Pearson comments guardedly (1967, 345):

The basis of the approach used here is a little obscure and there seems to be implicit in it the classical concept of inverse probability.

To provide a full exposition of Pearson and Filon (1898), showing the Bayesian nature of their argument, would be a lengthy, but relatively straightforward task (some useful clues are provided by Welch, 1958, 780). Instead, I shall present a special case of the general theorem constructed by Pearson and Filon, in order to show simply and briefly the nature of their approach. The problem I shall consider is that of finding the probable error of a standard deviation/...

deviation.⁽⁶⁾ (The general theorem gave a means of calculating the probable errors, and correlation of errors, of 'frequency constants' - parameters of frequency distributions - in general.)

Consider a simple random sample of size n drawn from a normally-distributed population with mean zero and unknown standard deviation θ : denote this sample x_1, x_2, \dots, x_n , or, collectively, \underline{x} . The probability of the occurrence of this sample, for a given value of θ , is given by

$$g(\underline{x}; \theta) = \frac{1}{(2\pi)^{n/2} \theta^n} \exp \left\{ -(\underline{x}_1^2 + \underline{x}_2^2 + \dots + \underline{x}_n^2) / 2 \theta^2 \right\}.$$

Pearson argued that the best value to give θ is that which maximises $g(\underline{x}; \theta)$.⁽⁷⁾ He showed that this value was θ_0 , where $\theta_0^2 = (\sum x^2) / n$. He then proceeded to evaluate the probable/...

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(6) Karl Pearson dealt with this problem in his lectures on statistical theory in the Autumn of 1894. His treatment of it is described in Yule's Notes (1, 89-92). From Yule's Notes alone it is not entirely clear what Pearson was doing; but when the relevant passage is interpreted in the light of the nineteenth century understanding of the 'method of inverse probability' the basis of the analysis becomes clear. The continuity between this example and the general theorem of Pearson and Filon (1898) is apparent: for example, a one-dimensional version of the general theorem is given in Yule's Notes (5, 41-2), in a passage dating from January 1896.

(7) The justification for this was apparently not discussed by Pearson at any length: it may have rested in part on the interpretation of $g(\underline{x}; \theta)$ as the posterior distribution of θ . One problem with the general theorem of Pearson and Filon (1898) is that it was in practice applied to estimates that were not maxima of the posterior distribution, in particular to estimates of the parameters of skew curves obtained by the method of moments; this was pointed out by R.A. Fisher (1922a, 329 fn).

probable error of the standard deviation as estimated by this method.

Pearson's procedure makes sense only if it is interpreted as an application of the traditional 'method of inverse probability'. It rested on the (implicit) treatment of $g(x; \theta)$ as the posterior distribution of θ : his manipulation of $g(\underline{x}; \theta)$ is meaningless if $g(\underline{x}; \theta)$ is interpreted as a direct, rather than an inverse, probability. Because θ_0 is the value of θ that maximises $g(\underline{x}; \theta)$, it is the mode of the posterior distribution of θ . Write θ as $\theta_0 + z$. Then

$$g(\underline{x}; \theta) = \frac{1}{(2\pi)^{n/2} (\theta_0 + z)^n} \exp \left\{ - (\sum x^2) / 2 (\theta_0 + z)^2 \right\}.$$

Now
$$g(\underline{x}; \theta_0) = \frac{1}{(2\pi)^{n/2} \theta_0^n} \exp \left\{ - (\sum x^2) / 2 \theta_0^2 \right\}.$$

So
$$g(\underline{x}; \theta) / g(\underline{x}; \theta_0) = (1 + z/\theta_0)^{-n} \exp \left\{ -\frac{n}{2} (1 + z/\theta_0)^{-2} + \frac{n}{2} \right\}$$

since $\sum x^2 = n \theta_0^2$. This is the posterior distribution of θ about its mode. It is clearly not normal. However, if we assume that the errors under consideration are small

(i.e. that z is small by comparison with θ_0), the distribution

can be shown to be approximately normal. For $(z/\theta_0) \ll 1$, we have

$$\begin{aligned} (1 + z/\theta_0)^{-n} &= \exp \left\{ -n \log (1 + z/\theta_0) \right\} \\ &= \exp \left\{ -n \left(z/\theta_0 - \frac{1}{2} z^2/\theta_0^2 + \frac{1}{3} z^3/\theta_0^3 - \dots \right) \right\} \end{aligned}$$

and
$$\exp \left\{ -\frac{n}{2} (1 + z/\theta_0)^{-2} + \frac{n}{2} \right\} = \exp \left\{ -n \left(-z/\theta_0 + \frac{3}{2} z^2/\theta_0^2 - 2 z^3/\theta_0^3 + \dots \right) \right\}.$$

Neglecting terms in $(z/\theta_0)^3$ and higher powers, we have,

therefore,

$$g(\underline{x}; \theta) / g(\underline{x}; \theta_0) = \exp \left\{ -n z^2 / \theta_0^2 \right\}$$

So/...

So the posterior distribution of θ is given by

$$\pi'(\underline{x}|\theta) = C g(\underline{x}; \theta) = \text{Constant} \times \exp \left\{ - \frac{n(\theta - \theta_0)^2}{\theta_0^2} \right\},$$

since $g(\underline{x}|\theta_0)$ is independent of θ and $z = \theta - \theta_0$.

This is a normal curve with mean θ_0 and standard deviation

$\theta_0/\sqrt{2n}$. The probable error of θ_0 is thus $0.6745\theta_0/\sqrt{2n}$:

that is to say, the posterior probability that the true value of θ lies in the interval $\theta_0 \pm 0.6745 \frac{\theta_0}{\sqrt{2n}}$ is 0.5.

The approach to finding the probable errors of frequency constants presented in Pearson and Filon (1898) was a generalisation of that employed in this example. They treated what we would now call the 'likelihood function' as the joint posterior distribution of the frequency constants in question; they were, therefore, following the 'method of inverse probability' and making the implicit assumption of a uniform joint prior distribution of the frequency constants. From this they showed that - on the assumption of 'small' errors - the joint posterior distribution was multivariate normal, and they gave an expression for what would now be called its covariance matrix, which enabled the probable errors and correlation of errors of frequency constants to be calculated.

Despite the impressive nature of this result, Pearson cannot have been entirely happy with the process by which it had been reached, for as soon as an alternative means of deriving probable errors became available, he adopted it. The alternative method was developed by W.F. Sheppard/...

W.F. Sheppard. Sheppard seems to have taken Venn's criticisms of the basis of the 'method of inverse probability' more to heart than did either Edgeworth or Pearson. Writing to Galton on 29 February 1896 about the paper that was to become his major contribution in this area (Sheppard, 1898b), Sheppard said:

In dealing with questions of probability I have proceeded on what appear to me to be the correct lines laid down by Venn in The Logic of Chance.
(Galton Papers, 315)

The key to Sheppard's approach was his non-Bayesian concept of 'probable error'. This he defined, not as an interval of a posterior probability distribution, but in terms of the distribution of an estimate in repeated sampling.

Pearson later summed up Sheppard's concept:

The simple idea involved in the probable error of a statistical constant is of the following kind: If the whole of a population were taken we should have certain values for its statistical constants, but in actual practice we are only able to take a sample, which should if possible be a 'random sample'. If a number of random samples be taken any statistical constant will vary from sample to sample, and its variation is distributed according to some law round the actual value of the constant for the total population. This variation would be very properly measured by the standard deviation of the constant for an indefinitely great series of random samples.
(K. Pearson, 1903b, 273)

By defining 'probable error' in these terms, Sheppard reformulated the problem in such a way as to avoid the use of any but 'direct' probabilities. He started with an analysis of a simple kind of repeated sampling:

Let the individuals comprised in an indefinitely great community be divided into any number of classes/...

classes A, B, C, ..., and let the numbers in these classes be proportional to $\alpha, \beta, \gamma, \dots$, so that $\alpha + \beta + \gamma + \dots = 1$. Suppose a random selection of n individuals to be made, and let the numbers drawn from the different classes be respectively $n\alpha', n\beta', n\gamma', \dots$, so that $\alpha' + \beta' + \gamma' + \dots = 1$. Then $\alpha - \alpha', \beta - \beta', \gamma - \gamma', \dots$ are the errors in $\alpha, \beta, \gamma, \dots$

(Sheppard, 1898b, 117; Sheppard's emphasis)

By using the multinomial distribution, Sheppard quickly derived the following results:

- (i) that the mean value of α' is α ;
- (ii) that the mean square of $\alpha' - \alpha$ is $\alpha(1 - \alpha)/n$;
- (iii) that the mean value of $\alpha'\beta'$ is $\alpha\beta - \alpha\beta/n$, and the mean value of the product of $\alpha' - \alpha$ and $\beta' - \beta$ is $-\alpha\beta/n$.

He showed that the general linear function $a(\alpha' - \alpha) + b(\beta' - \beta) + c(\gamma' - \gamma) + \dots$ has mean value zero and mean square $\{a^2\alpha + b^2\beta + c^2\gamma + \dots - (a\alpha + b\beta + c\gamma + \dots)^2\}/n$.

He gave expressions for its mean product with another similar linear function, and for its mean k th power, and showed that for large n its distribution tended to normality.

Sheppard then used these results to attack the main problem of the probable error of frequency constants:

Let X be any magnitude which is determined by observation of the ratios $\alpha', \beta', \gamma', \dots$. Then X can be written in the form $f(\alpha', \beta', \gamma', \dots)$. Now suppose n to be very great. Then the values of $\alpha - \alpha', \beta - \beta', \gamma - \gamma', \dots$ are distributed normally with mean values zero and mean squares $\alpha(1 - \alpha)/n, \beta(1 - \beta)/n, \gamma(1 - \gamma)/n, \dots$; and therefore it may be supposed that in any particular case the values of $\alpha' - \alpha, \beta' - \beta, \gamma' - \gamma, \dots$ will be very small. Thus X is of the form $f(\alpha, \beta, \gamma, \dots) + f_\alpha(\alpha' - \alpha) + f_\beta(\beta' - \beta) + f_\gamma(\gamma' - \gamma) + \dots$ (8) and/...

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- (8) [Sheppard, who was trying to avoid the explicit use of the differential calculus in this paper, did not define $f_\alpha, f_\beta, f_\gamma$, etc. They are, of course, the coefficients of the first order in the Taylor expansion of f .]

and therefore [by the above] its mean value is $f(\alpha, \beta, \gamma, \dots)$, and the different possible values are distributed normally about this mean value with mean square

$$\{ (\alpha f_{\alpha}^2 + \beta f_{\beta}^2 + \gamma f_{\gamma}^2 + \dots) - (\alpha f_{\alpha} + \beta f_{\beta} + \gamma f_{\gamma} + \dots)^2 / n \}$$

(Sheppard, 1898b, 122-3)

If the expression in the curled brackets be called σ^2 , and if Q be the factor 0.6745, then $Q\sigma/\sqrt{n}$ is the probable error of X , the probable discrepancy between its observed value $f(\alpha', \beta', \gamma', \dots)$ and its true value $f(\alpha, \beta, \gamma, \dots)$. As we will not in practice know the true values $\alpha, \beta, \gamma, \dots$, in evaluating $Q\sigma/\sqrt{n}$ we can only use the observed values α', β', γ' . 'But, n being great, the mistake so introduced in $Q\sigma/\sqrt{n}$ is small in comparison with $Q\sigma/\sqrt{n}$ itself'. (Sheppard, 1898b, 124).

Pearson soon adopted Sheppard's method. In his paper on the tetrachoric coefficient (1900b), the probable error of the coefficient is calculated by Sheppard's method. (It is, indeed, far from clear how one could apply the method of Pearson and Filon (1898) to this case.) That this was not an isolated departure from the 1898 method was shown by an editorial 'On the Probable Errors of Frequency Constants' in Biometrika (Pearson, 1903b). There, the two 'fundamental memoirs' cited are Pearson and Filon (1898) and Sheppard (1898b), but the method used is that of Sheppard, and not that of Pearson and Filon.

In many cases, the probable errors obtained by the old (Pearson and Filon) method and by the new (Sheppard) method are identical. Nevertheless, in certain cases (notably/...

(notably those of method-of-moments estimates of the parameters of skew frequency curves) the methods do not agree. In practice, Pearson used Sheppard's method in these problematic cases (Pearson, ed., 1914; see Fisher, 1922a, 329 fn.); but he never publicly discussed the reasons for his change of procedure. Nor did he generalise his use of Sheppard's method for this type of problem to a rejection of the Bayesian approach in general.⁽⁹⁾ It would appear that, uneasy as he may have been about Bayesianism, and ready as he was to adopt non-Bayesian methods such as Sheppard's when these were available, he could see no general replacement for Bayesianism. He justified his continued adherence to the traditional view of inference on explicitly pragmatic grounds:

If science cannot measure the degree of probability involved [in prediction from past to future experience] - so much the worse for science. The practical man will stick to his appreciative methods until it does, or will accept the results of inverse probability of the Bayes/Laplace brand till better are forthcoming. (K. Pearson, 1920a, 3)

Aside from Pearson's Bayesianism, another - better known - aspect of his view of inference needs to be discussed: the persistent assumption in his statistical theory of large sample sizes. This has been examined by E.S. Pearson, who/...

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- (9) Pearson might reasonably have argued - although I have no evidence that he did - that the general theorem of Pearson and Filon (1898) was correct, even if it had been incorrectly applied to estimates that were not maxima of the posterior probability distribution.

who notes that Karl Pearson considered that small-sample statistical work 'was dangerous and should be avoided' (E.S. Pearson, 1939, 378). What was at stake, however, was more than the cultivation of one area of statistical theory as against another. Large-sample assumptions formed the justification for a characteristic move in Pearson's statistical theory. In evaluating the probable error of a frequency constant, one typically obtains an expression involving the value of that constant and/or other frequency constants. Pearson (in common with Sheppard and other statistical theorists of the time) would then substitute the sample estimate of the frequency constant for its unknown population value: if the sample size was large enough, the error this would lead to would be sufficiently small to be unimportant. Such was Pearson's confidence in this process that he never systematically distinguished, in his notation, between population parameters and sample estimates of these. His faith in the generality of this process can be seen as at the root of his disagreement with R.A. Fisher as to what to do when, in performing a chi square test, the constants of the expected distribution are estimated from observed data. Fisher (1922b) had argued that the number of degrees of freedom of chi square should be reduced by one for each constant estimated from the observed distribution. Pearson replied that such a substitution of a sample for a population value was quite unproblematic:

... in a considerable number of cases [the]
sampled/...

sampled population is unknown to us; we have no direct means of finding μ_2, μ_4 etc. [the moments of the population]. What accordingly do we do? Why we replace the constants of the sampled population by those calculated from the sample itself, as the best information we have. And the justification of this proceeding is not far to seek. μ_s as found for the sample will only differ from the μ_s of the sampled population by terms of the order $1/\sqrt{n}$; for example if we are not dealing with small samples, and σ' be the standard deviation of the sample, σ' differs from σ by terms of the order $\sigma/\sqrt{2n}$ and accordingly the standard deviation of the mean is written σ'/\sqrt{n} when it is really σ/\sqrt{n} . This method of treating probable errors is universal in the case of fair sized samples to-day and scarcely needs justification... ... What we actually do is to replace the accurate value of χ^2 , which is unknown to us, and cannot be found, by an approximate value, and we do this with precisely the same justification as the astronomer claims, when he calculates the probable error on his observations, and not on the mean square error of an infinite population of errors which is unknown to him ...
(K. Pearson, 1922, 186-7; Pearson's emphasis)

The next generation of British statisticians could not, unlike Karl Pearson, accept this substitution of sample for population values as unproblematic. In the next section, the work of the first person who seriously questioned it, W.S. Gosset, will be discussed. It would appear that Karl Pearson never fully understood the basis for the concern of Gosset (and those who followed him). Perhaps this was because Karl Pearson, as a biometric statistician and eugenicist, moved in a world of large samples (E.S. Pearson, 1967, 349-50). For him, statistical inference was, in practice, inference based on large samples: he had no need to seek a more general theory of inference.

8.3 W.S. Gosset ('Student')

W.S. Gosset (1876-1937) spent the entirety of his working life in the employment of Arthur Guinness and Son, the famous brewers.⁽¹⁰⁾ Guinness was an early example of an 'agribusiness' firm, operating on a scale that came near to dominating the economy of Ireland. In 1880 Guinness bought over half the Irish barley crop (Lynch and Vaizey, 1960, 221). The scope and scale of its activities made possible what was, for Ireland or Britain, a relatively early employment of science to understand and systematise the traditional art of the brewer, and to improve the processes of production of the agricultural raw materials on which brewing depended. Gosset (who started work for Guinness in 1899) was one of a number of science graduates taken on for this task.

His career, quite apart from its specific interest from the point of view of the history of statistics, forms an interesting case study for the sociology of science. As against the theory of Kornhauser (1962), it illustrates the claim by Barnes (1971) that individuals do not necessarily experience conflict between the demands of an industrial organisation and the 'norms of pure science'. There is no evidence/...

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(10) The following draws chiefly on E.S. Pearson (1939) and on the fascinating unpublished material quoted therein. Biographical information on Gosset is contained also in McMullen (1939) and Fisher (1939).

evidence that Gosset chafed at having (presumably because of a company regulation) to publish under a pseudonym. Although offered at least one academic job (McMullen, 1939, 357) he preferred to remain with Guinness, rising to become in 1935 manager of the newly established Guinness brewery in London. He seems to have accepted quite unselfconsciously the practical demands (and profit-oriented considerations) of his working environment (see, for example, E.S. Pearson, 1939, 366 and 373). Following Barnes (1971), we can say that Gosset's scientific training left him with competences that he employed in response to the demands of his occupational role, but not with a set of internalised scientific norms that conflicted with those demands. Further, the case of Gosset illustrates the thesis of Ben-David (1960, especially 557-8) that innovations in science are frequently the work of individuals marginal to the established scientific community, 'role-hybrids' with academic training but practical concerns (see also Mulkay, 1974 and Edge and Mulkay, 1975).

Gosset, the son of a colonel in the Royal Engineers, was educated in mathematics and the natural sciences at Oxford. He was apparently the most mathematical of the scientists employed by the company (McMullen, 1939, 355), and quickly became involved in the problems of analysis posed by the numerical results of experimental trials. His training had provided him with some resources applicable in this area, those of the theory of errors. E.S. Pearson (1939/...

(1939, 363) suggests that he used three texts of error theory: Airy (1861), Merriman (1901) and Lupton (1898). Gosset soon found, however, that error theory was not fully adequate for the kind of work that had to be done in the brewery.

One problem was the assumption made by the error theorists of the independence of observations (see section 2.4). As Gosset was later to put it, in the brewery situation, where many variables could not be controlled, 'secular change' could lead to 'successive experiments being positively correlated' (1908a, 12). He concluded (presumably as a result of direct experience) that the standard methods of combining independent errors were inapplicable (E.S. Pearson, 1939, 364-5). He was at this stage unaware of the work of Galton and Pearson on correlation, and could not entirely to his satisfaction solve the problem of how 'to establish a relationship between sets of observations' (E.S. Pearson, 1939, 366).⁽¹¹⁾ A second difficulty was, in essence, a problem of/...

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(11) E.S. Pearson suggests that 'given a little more time... Gosset would have found for himself Galton's correlation coefficient' (1939, 366). This seems to me unlikely. In one sense, Galton and Gosset shared a common problem: the inapplicability of the standard error theory techniques when dealing with dependent variables. But Galton's thought was informed by his work on heredity, and it was from this that he drew the key notion of reversion. Gosset does not seem to have had available to him a comparable intellectual resource, and so he approached the problem differently: unlike Galton, he worked directly from the 'anomaly' that arose in the application of error theory to brewery data. He deduced from this a criterion to help him judge whether or not two variables were independent, but apparently did not go on to seek a measure of the degree of their dependence. See the passage 'What is the right way to establish a relationship between sets of observations?', quoted by E.S. Pearson (1939, 366).

decision theory. In a report to his board of directors in 1904, he pointed out that because of the large scale processes used in brewing, and the difficulties of exact control of them, accurate experimentation was not possible, and any conclusions drawn were necessarily probabilistic rather than certain (E.S. Pearson, 1939, 363-4). The question was, then, that of the 'degree of probability to be accepted as proving various propositions' (E.S. Pearson, 1939, 365). Gosset soon realised that there was no single answer to this question:

... in such work as ours the degree of certainty to be aimed at must depend on the pecuniary advantage to be gained by following the result of the experiment, compared with the increased cost of the new method, if any, and the cost of each experiment.
(E.S. Pearson, 1939, 365-6)

The error theorists, working in astronomy and such fields, had not faced difficulties of this nature. This problem was the first posed by Gosset to Karl Pearson when he consulted him in July 1905, contact having been made through Vernon Harcourt, an Oxford chemist (E.S. Pearson, 1939, 365).

What advice, if any, Pearson was able to give Gosset on this question is unknown. Pearson was, however, able to solve Gosset's problem with non-independent observations by introducing him to the correlation coefficient. On his return to Dublin, Gosset enthusiastically applied the new method. As Egon Pearson puts it (1939, 367):

It became possible to assess with precision the relative importance of the many factors influencing quality at the different stages in the complicated process of brewing, and before/...

before long the methods of partial and multiple correlation were mastered and applied [by Gosset].

But a further problem had already arisen by the time Gosset met Pearson, and this was one for which there was no solution in either error theory or biometric statistical theory.

I find out the P.E. [probable error] of a certain laboratory analysis from n analyses of the same sample. This gives me a value of the P.E. which itself has a P.E. of $P.E. / \sqrt{2n}$. I now have another sample analysed and wish to assign limits within which it is a given probability that the truth must lie. E.g. if n were infinite, I could say 'it is 10 : 1 that the truth lies within 2.6 of the result of the analysis'. As however n is finite and in some cases not very large, it is clear that I must enlarge my limits, but I do not know by how much.

(E.S. Pearson, 1939, 366)

Error theorists such as Merriman (on whose work Gosset seems to have been drawing in this instance) were certainly aware that formulae such as that for the probable error of a probable error were strictly valid only for large numbers of observations. Nevertheless, they continued to use them: Merriman (1901) gives probable errors based on five, seven and eight measurements. In practice, of course, the error theorists were concerned chiefly with giving a fairly rough indication of the reliability of a result. It may not have worried them that exact probability statements based on the law of error were not valid for probable errors obtained from small numbers of observations. But it did worry Gosset, perhaps because of his 'decision theory' orientation. Furthermore, Gosset soon realised that precisely the same problem arose with the correlation methods he had so recently learnt:

Correlation/...

Correlation coefficients are usually calculated from large numbers of cases, in fact I have only found one paper in Biometrika of which the cases are as few in number as those at which I have been working lately.

(E.S. Pearson, 1939, 367)

Gosset was afraid that the biometric school's formula for the probable error of a correlation coefficient would not be valid for the small samples (e.g. $n = 10$) with which he had to deal.

Guinness must have been an unusually enlightened employer, for it was its 'general practice' to allow its scientific staff leave for specialised study (E.S. Pearson, 1939, 369). As a result of this arrangement, Gosset was able to spend the year 1906-7 in England, working with Karl Pearson. He spent the time learning mathematical statistics, and trying to find a solution to what he had come to see as the chief problem in using statistical methods in the brewery: the large-sample assumptions upon which the error theory and biometric techniques were based. Gosset wrote to a colleague in Ireland from his house in Tunbridge Wells:

I go up to K.P.'s lectures from here and on other days work at small numbers: a greater toil than I had expected, but I think absolutely necessary if the Brewery is to get all the possible benefit from statistical processes.
(E.S. Pearson, 1939, 373)

The result of his studies he published in two papers on 'The Probable Error of a Mean' and the 'Probable Error of a Correlation/...

Correlation Coefficient' (1908a; 1908b).⁽¹²⁾

Gosset began 'The Probable Error of a Mean' by pointing out that one can consider a series of experiments as a sample drawn from a hypothetical infinite population of experiments carried out under similar conditions. The statistician's aim was to 'form a judgment' about this population: typically this question 'turns on the value of a mean, either directly, or as the mean difference between ... two quantities' (1908a, 11). With only a small series of experiments to go on, there were two sources of uncertainty. One was uncertainty about the form of the population distribution; Gosset claimed that the assumption of normality was justified by experience, convenience and the fact that 'it appears probable that the deviation from normality must be very extreme to lead to serious error' (1908a, 11). This left him free to concentrate on the second source of uncertainty: the effect of 'errors of random sampling' on the reliability of estimates of the population mean.

Gosset pointed out that the standard method of treating this problem was based on the assumption of large sample size. This assumption was used to justify substituting the sample standard deviation for that of the population/...

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(12) In fact a third remarkable paper came out of his year's work, again a solution to a practical problem of the brewery. Gosset independently reinvented the Poisson distribution in studying the error involved in counting yeast cells with a haemocytometer (1907).

population in the formula for the probable error of a mean.

But large samples could not always be obtained:

... it is sometimes necessary to judge of the certainty of the results from a very small sample, which itself affords the only indication of the variability. Some chemical, many biological, and most agricultural and large-scale experiments belong to this class, which has hitherto been almost outside the range of statistical inquiry.
(1908a, 12)

His approach to the problem was in three stages. In the first, which proved the most difficult, he tried to find the sampling distribution of the square of the sample standard deviation (s^2) for a given value (σ^2) of the square of the population standard deviation. He was unable to provide a full analytic solution to the problem, but, by the use of Pearson's method of moments, he in fact reached the now accepted result (where n is the sample size and C a constant):

$$C (s^2)^{\frac{n-3}{2}} \exp \left\{ -\frac{n}{2} \frac{s^2}{\sigma^2} \right\} d(s^2). \quad (13)$$

In the second stage, he showed (denoting by x the difference between the sample mean and population mean) that x and s , and x^2 and s^2 , were both uncorrelated. In the third stage, he went on to make the implicit assumption that x and s were independent (not in fact strictly proven by the result of the/...

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- (13) In other words, ns^2/σ^2 is distributed as χ^2 with $n-1$ degrees of freedom. The distribution of s^2 had been discussed earlier by writers in the error theory tradition (see K. Pearson, 1931; Sheynin, 1966; Sheynin, 1971a; Kendall, 1971), but this work, on a problem that was marginal to the main concerns of error theory, was unknown to the British statisticians of the time.

the second stage), and to find the distribution of $z = x/s$, showing it to be:

$$\text{Constant } X \quad (1 + z^2)^{-\frac{1}{2}n} \quad dz,$$

where the constant depends on the value of n .⁽¹⁴⁾

Gosset concluded:

Since this equation is independent of σ it will give the distribution of the distance of the mean of a sample from the mean of the population expressed in terms of the standard deviation of the sample for any normal population.
(1908a, 18)

Gosset tested his equations for the distributions of s^2 and z by showing that they fitted well the observed distributions of artificially generated samples from a known population, and gave a table of z for n from four to ten. The paper concluded with examples of the use of the z distribution, taken from the testing of the efficacy of soporific drugs, and from agricultural research.

'The Probable Error of a Correlation Coefficient'

addressed itself to the following problem:

A random sample has been obtained from an indefinitely large population and r [the sample correlation coefficient] calculated between two variable characters of the individuals composing the sample. We require the probability that R [the correlation coefficient] for the population from which the sample is drawn shall lie between any given limits.
(Gosset, 1908b, 35)

To/...

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(14) The modern approach uses not z but $t = z (n - 1)^{\frac{1}{2}}$; making this substitution in the above equation immediately yields the t distribution with $n-1$ degrees of freedom.

To solve this problem two things had to be known, said Gosset: the distribution of r for a given value of R , and the prior distribution of R . The latter 'can hardly ever be known', he said, 'so that some arbitrary assumption must in general be made' (1908b, 35). His paper focused on the first issue. The distribution of r had been discussed by the biometricians, but

... their method involves approximations which are not legitimate when the sample is small.
(1908b, 36)

Gosset approached the problem empirically, taking a known population with $R = 0$, and examining the distribution of r in artificially generated samples of sizes two, four and eight. He found that the distribution

$$y_0 (1 - r^2)^{\frac{n-4}{2}} dr,$$

where y_0 is a constant, described adequately his empirical sampling distributions and, for large n , gave the results obtained by the biometricians. He concluded that it

... probably represents the theoretical distribution of r when samples of n are drawn from a normally-distributed population with no correlation.
(1908b, 41)

Gosset was, however, unable to suggest an extension of this result for populations with non-zero correlations.

These two papers represented far more than simply new results extending the scope of existing biometric theory. They can be seen as indicative of a different theoretical approach/...

approach. Unlike, for example, Pearson and Filon (1898), Gosset systematically distinguished between what would now be called sample statistics and population parameters, and used different notations for each (r and R , s and σ). He could not accept Pearson's view that the substitution of a sample estimate for a population parameter was unproblematic. Further, Gosset's focus on small-sample problems forced him to seek the exact distributions of sample statistics, rather than to rely on the asymptotic normality of sampling distributions. But if Gosset's work was in this sense a radical innovation, in a second sense it remained within the traditional approach to inference. Basically, Gosset's approach was Bayesian. This is explicit in Gosset (1908b), but can be seen also in the way in which Gosset refers (1908a, 30) to 'the chance that the mean of the population of which these experiments are a sample is positive'.

The important impetus given to the development of the theory of inference by Gosset can, as the above account has hopefully shown, be accounted for by Gosset's practical concerns. This was certainly Gosset's own view. He wrote to R.A. Fisher in 1915:

I don't know if it would interest you to hear how these things came to be of interest to me but it happened that I was mixed up with a lot of large scale experiments partly agricultural but chiefly in an Experimental Brewery. The agricultural (and indeed almost any) Experiments naturally required a solution of the mean/S.D. problem and the Experimental Brewery which concerns such things as the connection between analysis of malt and hops, and the behaviour of the beer, and which takes a day to each unit of the experiment, thus limiting the numbers, demanded an answer to such questions as 'If with a small number of cases I/...

I get a value r , what is the probability that there is really a positive correlation of greater than (say) .25?'

(Quoted by E.S. Pearson, 1968, 407)

It would of course be mistaken to see Gosset's practical concerns generating his results ab nihilo: he drew fully on the established corpus of biometric theory. Both Gosset (1908a) and (1908b) rely heavily on, for example, Pearson's system of frequency curves (Pearson, 1895). In the case of Gosset, we have an instance of how a new focus for cognitive interests in prediction and control - the sphere of agricultural and industrial production - could transform an existing corpus of knowledge.

As E.S. Pearson has shown, the practitioners of the existing corpus of knowledge were, while not hostile, unimpressed. Thus, Karl Pearson, writing to Gosset on 17 September 1912 on the issue of whether to estimate the standard deviation by dividing the sum of squared deviations by n or by $n - 1$, remarked that the issue was of little practical importance

... because only naughty brewers take n so small that the difference is not of the order of the probable error!

(Quoted by E.S. Pearson, 1939, 368)

Lacking Gosset's practical concerns, Karl Pearson could see the problem of small samples as, at best, only a marginal one. The integration into statistical theory of Gosset's pioneering insights was to be achieved not by Pearson, but by R.A. Fisher.

8.4 R.A. Fisher as 'Metastatistician'

When Fisher became interested in statistical theory in the years immediately preceding 1914, he faced a very different situation from that faced by Pearson 20 years earlier. In the early 1890's there was no coherent tradition of statistical theory in Britain. Statistical inference as a field of study hardly existed. Although the method of inverse probability had been challenged on philosophical grounds, no practical alternative existed. The use of the normal distribution, especially by the error theorists, dominated statistical practice. Although a little work had been done on non-normal distributions, the normal distribution continued to hold theoretical sway. By around 1911, however, a network of statisticians had come into existence, with personal links established even between those (such as Pearson and Gosset) whose practical concerns were different. Sheppard had shown that non-Bayesian inference was a practical possibility. Pearson had extensively discussed frequency curves other than the normal, and several non-normal distributions (notably chi square, but also Gosset's 'z' and the as yet not fully understood sampling distribution of the correlation coefficient) had received theoretical attention. A much richer intellectual field thus lay before Fisher, and the network of personal contacts between statisticians was such that (unlike in the case of Arthur Black in the 1880's) intellectual isolation was not a major problem.

No/...

No immediate practical concern appears to have generated Fisher's work on statistical inference. It is not that Fisher was, by nature, a 'pure theorist'. As discussed above, his concern for eugenics was vitally important to his work on evolution and genetics. It is rather that the social and intellectual development of British statistics had, by 1914, opened up the opportunity for the successful pursuit of 'metastatistics'.⁽¹⁵⁾ Fisher's 'metastatistics' was a 'second-level' activity: solving problems thrown up by the work of others, criticising others' procedures, attempting to integrate existing techniques into a systematic theory. If a single characterisation of Fisher's 'metastatistics' of this period was required, it would be this: the development of a non-Bayesian theory of inference and the consequent reformulation of the 'mathematical foundations of theoretical statistics', to quote the title of the crucial paper (1922a) which formed the culmination of Fisher's early work on inference.

The origins of Fisher's anti-Bayesian outlook are, at present, unknown; until the Fisher papers are opened/...

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- (15) In a sense Edgeworth had, much earlier, been a 'metastatistician', but the intellectual resources for metastatistics in the 1880's and 1890's were relatively sparse. The real advances were made by men such as Galton and Pearson, whose practical concerns led them to extend statistical theory in new directions, and thus to add to it, rather than reflect on it.

opened any statement on this must be premature.⁽¹⁶⁾ There can, however, be little doubt that by around 1911 Bayesianism in Britain was on the defensive. As shown in section 8.2, even Pearson, who continued to be an advocate of Bayesianism, clearly felt uneasy about the basis of Bayesian techniques. In an intellectual climate where empiricism and positivism were commonplace, Bayesianism, with its apparently weak empirical base, came under suspicion. Fisher's key role was to turn this suspicion into a viable alternative to Bayesianism.⁽¹⁷⁾

In this section I shall trace in outline the development of Fisher's work on inference from his first paper in the theory of statistics (1912c) to his crucial (1922a). Aside from showing how Fisher attempted to construct his non-Bayesian theory of inference, this section will examine the role of the intellectual resources provided by/...

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(16) John Venn was President of Gonville and Caius College when Fisher was an undergraduate there. Despite Venn's age (he was 75 in 1909), personal contact between him and Fisher cannot be ruled out, and Fisher would almost certainly have read Venn's Logic of Chance. It may have been through Venn, therefore, that Fisher first came into contact with explicitly anti-Bayesian ideas. But the mere fact of contact would not explain Fisher's adoption of these ideas.

(17) Of course, a Bayesian (e.g. Jeffreys, 1974) would argue that Fisher's approach was not a real alternative to Bayesianism, but simply one in which particular assumptions were hidden from view. Given the incommensurability of the Bayesian and frequentist frameworks, this is an impossible issue to adjudicate. The historically important point is, however, that Fisher felt he was developing a radically non-Bayesian theory of inference. For an interesting evaluation of Fisher by a leading modern Bayesian, see Savage (1976).

by the previous generation of British statisticians in Fisher's 'metastatistics'. The section will end with a brief examination of the dispute between Fisher and Karl Pearson.

Fisher (1912c) took up a problem prominent in the early statistical work of Karl Pearson: the fitting of frequency curves to observed data. Pearson (1895) had provided a practical solution to the problem, the 'method of moments', in which the moments of the observed data would be calculated, and used to choose which of Pearson's family of frequency curves best fitted the data and what values the parameters of the chosen curve should be given. Fisher, however, felt that Pearson's method lacked any clear theoretical justification, and sought instead an 'absolute criterion'. At first sight, Fisher's method looked like the old 'method of inverse probability'. Suppose we are trying to fit a curve of the form $f(x, \theta_1, \dots, \theta_r)$ to a set of data (x_1, \dots, x_n) , where $\theta_1, \dots, \theta_r$ are parameters whose best values we are seeking. Fisher suggested that the best values of $\theta_1, \dots, \theta_r$ were those which maximised P , where

$$\log P = \sum_{i=1}^n \log f(x_i, \theta_1, \dots, \theta_r).$$

On the 'method of inverse probability' P is simply the posterior probability of $\theta_1, \dots, \theta_r$ (on the assumption of a uniform joint prior distribution of these parameters). Indeed, Fisher wrote 'the probability of any particular set of θ 's is proportional to P ' (1912c, 157). On the last page/...

page of his paper, however, there was to be found a striking passage which showed clearly his divergence from the 'method of inverse probability':

We have now obtained an absolute criterion for finding the relative probabilities of different sets of values for the elements of a probability system of known form. It would now seem natural to obtain an expression for the probability that the true values of the elements should lie within any given range. Unfortunately we cannot do so. The quantity P must be considered as the relative probability of the set of values $\theta_1, \theta_2, \dots, \theta_r$; but it would be illegitimate to multiply this quantity by the variations $d\theta_1, d\theta_2, \dots, d\theta_r$, and integrate through a region, and to compare the integral over this region with the integral over all possible values of the θ 's. P is a relative probability only, suitable to compare point with point, but incapable of being interpreted as a probability distribution over a region, or of giving any estimate of absolute probability.

This may be easily seen, since the same frequency curve might equally be specified by any r independent functions of the θ 's, say $\phi_1, \phi_2, \dots, \phi_r$, and the relative values of P would be unchanged by such a transformation; but the probability that the true values lie within a region must be the same whether it is expressed in terms of θ or ϕ , so that we should have for all values

$$\frac{\partial(\theta_1, \theta_2, \dots, \theta_r)}{\partial(\phi_1, \phi_2, \dots, \phi_r)} = 1$$

a condition which is manifestly not satisfied by the general transformation.
(1912c, 160)

What Fisher was pointing out in the above passage was that, used in different ways, the standard method of inverse probability (which he described in the first paragraph above) gave different results. Let Ω be a section of the parameter space of the θ 's, and Ω' the corresponding section of the parameter space of the ϕ 's, and let

$$J = \frac{\partial(\theta_1, \theta_2, \dots, \theta_r)}{\partial(\phi_1, \phi_2, \dots, \phi_r)}$$

be the Jacobian of the transformation from the θ 's to the ϕ 's/...

ϕ 's. Then, by a standard formula of the integral calculus,

$$\int_{\mathcal{N}} P d\theta_1 \dots d\theta_r = \int_{\mathcal{N}'} P J d\phi_1 \dots d\phi_r,$$

and thus the probability that the value of the ϕ 's lies in \mathcal{N} is given by

$$\int_{\mathcal{N}'} P J d\phi_1 \dots d\phi_r.$$

However, applying the method of inverse probability directly to the ϕ 's, that probability is given simply by

$$\int_{\mathcal{N}'} P d\phi_1 \dots d\phi_r.$$

The two results are thus compatible only if $J = 1$, which, as Fisher pointed out, would not in general be the case.

The paradox arises because the two different applications of the method of inverse probability involve two contradictory assumptions: in the first case of a uniform joint prior distribution of the θ 's, in the second of a uniform joint prior distribution of the ϕ 's. Phrased in this way, Fisher's result is hardly surprising, but we must remember that assumptions of uniform prior distributions were at the time often made as a matter of course, and non-uniform priors were seldom considered. Fisher was pointing out that the method of inverse probability as it was in fact used (with a more or less automatic assumption of uniform priors), could not provide consistent results. One way of resolving this problem would have been to advocate much more careful consideration of prior distributions (including the use of non-uniform priors). Fisher, however, was to choose a different way: that of attempting to avoid completely the assumption of prior distributions.

Gosset/...

Gosset was the only practising statistician who seems to have read Fisher's paper when it was published. He thought Fisher's approach 'unpractical and unserviceable' and wrote to Fisher making some particular criticisms. It appears to have been in pondering these that Fisher came up with one of his most important ideas: that of representing a sample of size n by a point in n -dimensional space. By the end of the Summer of 1912 he had written to Gosset giving, by use of his method, a proof of Gosset's z -distribution.⁽¹⁸⁾ Two years later he had solved by this method an even more difficult problem, that of the sampling distribution of the correlation coefficient. The biometricians had tried unsuccessfully to solve this problem (Soper, 1913): it was of greater import to them than that of the distribution of z , because the latter distribution rapidly approached the normal, while that of r appeared to do so only much more slowly and for very high values of the correlation coefficient the deviation from normality might be important even for the large sample sizes with which the biometricians worked. Fisher's work on these two problems was published in Biometrika (Fisher, 1915): this paper sufficed to establish him in the eyes of Pearson and Gosset as a mathematical statistician of note.

From 1915 there is a break of five years in Fisher's publications/...

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(18) Gosset described his early contacts with Fisher in a letter to Karl Pearson, 12 September 1912, quoted by E.S. Pearson (1968, 406).

publications in statistical theory proper. It can be presumed, however, that he continued to be active in working in the field, for his next three papers on inference (1920; 1921; and, especially, 1922a) were major contributions to statistical theory. The themes of the earlier papers (1912c; 1915) were taken up, generalised and formalised in a way that diverged in two crucial ways from 'traditional' and biometric approaches to inference.

Firstly, these papers generalised the anti-Bayesianism of Fisher (1912c). In part in response to misunderstanding of his approach by the biometricians, Fisher clarified the divergence between it and the 'method of inverse probability'.

The attempt made by Bayes, upon which the determination of 'inverse probabilities' rests, admittedly depended upon an arbitrary assumption, so that the whole method has been widely discredited ... two radically distinct concepts have been confused under the name of 'probability' and only by sharply distinguishing these can we state accurately what information a sample does give us respecting the population from which it is drawn.
(1921a, 4-5)

Ordinary direct probability - which Fisher defined in terms of relative frequency (1922a, 312) - was, for example, what one used in describing the probability distribution of a sample correlation coefficient r for a definite value of the correlation, ρ , of the population from which it was drawn. When, however, one discussed the unknown value of ρ on the basis of knowledge of a particular sample with a correlation of r , it was impossible to find a probability distribution/...

distribution of ρ :

Such a problem is indeterminate without knowing the statistical mechanism under which different values of ρ come into existence; it cannot be solved from the data supplied by a sample, or any number of samples, of the population.
(1921a, 24)

The best one could do was to find what Fisher had in 1912 called the 'relative probability' of different values of ρ , and now called their 'likelihood':

What we can find from a sample is the likelihood of any particular value of ρ , if we define the likelihood as a quantity proportional to the probability that, from a population having that particular value of ρ , a sample having the observed value r , should be obtained.
(1921a, 24)

The concepts of probability and likelihood were, Fisher said, radically different in their nature.

We may discuss the probability of occurrence of quantities which can be observed or deduced from observations, in relation to any hypotheses which may be suggested to explain these observations. We can know nothing of the probability of hypotheses or hypothetical quantities. On the other hand we may ascertain the likelihood of hypotheses and hypothetical quantities by calculation from observations: while to speak of the likelihood (as here defined) of an observable quantity has no meaning.
(1921a, 25)

Later Fisher was to qualify (with his concept of 'fiducial probability') the radicalism of his denial of the meaningfulness of probability statements about hypotheses. Nonetheless, this paper is of vital importance, constituting as it did the first clear public rejection by a practising British statistician of Bayesian methods.

The second divergence from tradition was the emphasis/...

emphasis placed by Fisher on the construction of exact distributions. In the work of Gosset this emphasis had, as we have seen, arisen from practical concerns. In Fisher's approach it was given a new theoretical centrality. In his opinion, the job of the statistician could be resolved into three parts. The statistician should treat any set of data as a sample from a - possibly hypothetical - population. The first problem faced by the statistician was that of deciding what mathematical form the distribution of the population should be assumed to take. This should initially be done on a pragmatic and empirical basis, and the assumptions made tested later. The second problem was that of the estimation, from the sample data, of the parameters of this population distribution (for example, of its mean and standard deviation if it were a normal distribution). The third part of the statistician's job Fisher summed up as 'problems of distribution', that is problems of the discovery of the sampling distributions of the 'statistics' used to estimate the population parameters. This was of crucial importance, because only through the knowledge of these sampling distributions could estimation be changed from a matter of common-sense to one of science:

... the study of the random distribution of different suggested statistics, derived from samples of a given size, must guide us in the choice of which statistic it is most profitable to calculate.

(Fisher, 1922a, 314)

Fisher suggested three 'criteria of estimation' (1922a, 316).

The first was 'consistency':

That/...

That when applied to the whole population the derived statistic should be equal to the parameter.

The second was 'efficiency':

That in large samples, when the distribution of the statistics tend to normality, that statistic is to be chosen which has the least probable error.

The third was that of 'sufficiency':

That the statistic chosen should summarise the whole of the relevant information supplied by the sample.

This latter criterion had emerged in Fisher's discussion (Fisher, 1920) of different means of estimating the standard deviation of a normal population. Fisher had shown that the formula,

$$\sigma_2 = \sqrt{\frac{\sum (x - \bar{x})^2}{n}},$$

was more 'efficient' (in the above sense) than its competitor, Bessel's formula,

$$\sigma_1 = \sqrt{\frac{\pi}{2}} \frac{\sum (|x - \bar{x}|)}{n}.$$

This result had been known to at least some error theorists. What was new was that Fisher showed that for a given value of σ_2 , the distribution of σ_1 was independent of the value of the population standard deviation σ , and thus 'the actual value of σ_1 can give us no further information as to the value of σ ' (Fisher, 1920, 768).

Fisher (1922a) was chiefly important in its systematic presentation of Fisher's new formulation of the tasks of statistical theory. But Fisher by no means restricted himself to the presentation of definitions. In a section entitled 'Formal Solution of Problems of Estimation' he returned to the method of Fisher (1912c), now described as/...

as the 'method of maximum likelihood'. He carefully distinguished likelihood from inverse probability, and solved a problem which had not been raised in his previous work: that of providing a general expression for the 'standard error'⁽¹⁹⁾ of statistics obtained by the method of maximum likelihood. Suppose the population distribution of a variable x depends on a single parameter θ : call this distribution $f(x, \theta)$. Let $\hat{\theta}$ be the maximum likelihood estimator of θ . Then $\sigma_{\hat{\theta}}$, the standard error of $\hat{\theta}$, is given, Fisher (1922a, 327-9) showed, by the formula

$$-\frac{1}{\sigma_{\hat{\theta}}^2} = n \left\{ \text{mean value of } \frac{\partial^2 \log f(x, \theta)}{\partial \theta^2} \right\}.$$

This result was reached by a process analytically very similar to that of Pearson and Filon (1898), but interpreted in terms of relative frequency, not inverse probability.⁽²⁰⁾ Fisher went on to argue (incorrectly, as he was later to acknowledge) that maximum likelihood estimators generally satisfy the criterion of sufficiency.⁽²¹⁾ The paper ended with an extensive discussion of various practical applications of the new approach: for example, Fisher showed that Pearson's 'method/...

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- (19) In this period 'standard error' (standard deviation of the sampling distribution) was gradually supplanting the older 'probable error'. The first use of the former term was apparently by Yule (1897a, 483; see Walker, 1929, 188). In part the change reflected the general prominence the biometricians gave to the standard deviation; in part, the increasing tendency to think of the 'error' as an interval of a sampling distribution, not of a posterior probability distribution.
- (20) Fisher held (1922a, 329 fn.) that the Pearson/Filon approach was correct so long as it was interpreted as giving the standard errors of maximum likelihood estimators.
- (21) In fact, Fisher proved the converse, that sufficient statistics, when they exist, are maximum likelihood estimators (1922a, 330-1).

'method of moments' was not in general 'efficient'.

Fisher's reformulation of statistical theory did not depend logically on the work of the previous generation of British statisticians. However, it seems unlikely that without this previous generation Fisher would ever have been able to do this work, for without them there would have been precious little, in terms of ongoing activity, to reformulate. Moreover, although it is impossible without more evidence to be certain of the detailed genesis of Fisher's concepts, it seems likely that they arose in part in consideration of particular problems. Consideration of Pearson's method of moments seems to have played an important role in the evolution of the method of maximum likelihood. It is unlikely that Fisher would have focused in the way he did on exact distributions, had he not had two partially worked out exemplars (Gosset's studies of the distributions of 'z' and 'r') before him. The concept of sufficiency arose from consideration of a long-established problem of error theory. Thus, in a certain sense, the preconditions for Fisher's 'metastatistics', in terms of a relatively rich body of statistical practice and partially theorised techniques, had been laid down by the work of men like Pearson and Gosset: their work was a vital resource for his.

'Metastatistics' of this kind had, inevitably, a critical edge. Inadequacies in the statistical practice and crudity in the statistical theory of the previous generation were highlighted by Fisher. They, by and large, had/...

had been men concerned with the development of adequate tools for tasks defined largely by their extra-statistical concerns: Fisher, by comparison, studied the tools in themselves and their relations to each other. Part of the dispute that arose between Fisher and Pearson was simply misunderstanding, as when Pearson interpreted Fisher's application of his 'absolute criterion' to sample estimates of the correlation coefficient as a Bayesian argument involving a uniform prior distribution.⁽²²⁾ More serious in their effects were Fisher's direct criticisms of some of Pearson's methods, notably the method of moments (see above) and the choice of degrees of freedom for the chi square test when/...

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- (22) Fisher (1915, 520-1); Soper et al. (1917); Fisher (1921a). It is interesting that the connection between biometric statistics and eugenics may have led the biometricians to feel that the assumption of a uniform prior in the case of the coefficient of correlation was unjustified. They gave a practical example of how Bayesian inference might be used in the case of a researcher confronted with a parent-child correlation of 0.6 based on a sample of 25. They argued that, from their considerable experience of such correlations, a suitable prior distribution would be one which had a mean of 0.46 and a standard deviation of 0.02. The 'most likely value' of the correlation coefficient of the population from which the sample was drawn they found using Bayes's theorem to be 0.46225. 'We see that our new experience scarcely modifies the old and this is what we should naturally conjecture would be the case' (Soper et al., 1917, 359). In view of the analysis in chapter seven, this conclusion is interesting as representing formal evidence of the biometricians' degree of belief in the clustering of the values of parent-child correlations.

Gosset had also found that the use of non-uniform prior distributions for the correlation coefficient 'made a fool of the actual sample'. For him, however, this was an argument against their use, and in favour of Fisher's approach. See Gosset's letter to Fisher 3 April 1922, in McMullen (ed.) (n.d.).

when applied to a two-way table. Pearson had argued (1900c, 164-7) that in cases (of which the two-way table was one) where the parameters of the theoretical frequency distribution were estimated from the data, the chi square test could be used without alteration. Fisher (1922b) argued that the degrees of freedom of chi square were reduced by one for each parameter estimated from the data. For a two-by-two table, Fisher concluded, the correct number of degrees of freedom was not three, as Pearson had assumed, but one.

The dispute between Pearson and Fisher cannot be seen (as that between Pearson and Bateson, say, can be) as resulting from incompatible cognitive interests. One is tempted to say, in the case of the degrees of freedom for chi square, that Pearson was simply mistaken.⁽²³⁾ More generally, it can indeed be said that Pearson and Fisher differed in their approaches to inference (E.S. Pearson, 1936-8, part 1, 222-3 and part 2, 211-3). Yet it still makes sense to see both approaches as embodying the same cognitive interest in inference as a means of prediction and control. Both Pearson's and Fisher's approaches can be seen as attempts to generalise from the known to the unknown, ultimately with a view to maximising man's potential control over social and natural processes. The difference between/...

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(23) E.S. Pearson (1936-8, part 1, 222) has pointed to the precise lacuna in Pearson's analysis. Yule had already seen it in 1916: see his letter to Greenwood of 12 March 1916 (Yule-Greenwood Letters).

between the two approaches lies perhaps in this: that Pearson was concerned primarily with prediction and control in a limited area, and was content with what seemed to him to be a theory of inference adequate to this task, while Fisher in his 'metastatistics' turned to problems of prediction and control more generally.

Two consequences flow from this. The first is that to some extent a model of cumulative growth, rather than of incommensurability in the full sense, must be seen as applying to development of statistics from the biometric to the 'Fisherian' paradigm.⁽²⁴⁾ Acceptance of Fisher's approach did not necessarily entail discarding the theoretical work of the biometricians; the practical inferences made with biometric statistics could (perhaps with minor modifications) still be made using Fisher's approach. Thus, Fisher did not say that the method of moments was wrong: merely that it was not always the most efficient. He did not replace the chi square test, but simply suggested how it could more accurately be used.⁽²⁵⁾ The second consequence was that the Pearson/Fisher controversy largely lacked the 'group' structure characteristic of the debates discussed in/...

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(24) Except in as far as the biometric approach was Bayesian, in which case one can talk of incommensurability; but the biometricians were only half-heartedly Bayesian.

(25) Compare the divergence between Pearson and Yule, where acceptance of the approach of one side entailed discarding the techniques of the other side virtually in their entirety, and often rejecting the concrete inferences of the other side.

in chapters six and seven. Pearson was, in effect, isolated. The older generation of statisticians accepted, albeit with reluctance,⁽²⁶⁾ that Fisher, in relation to Karl Pearson, was 'right'. Fisher's approach was more general and more powerful.⁽²⁷⁾ Individual explanation, rather than social-structural explanation, seems apt in this case. Thus, one might hypothesise that Pearson's personal psychological 'investment' in the particularities of the techniques that he developed was too great to allow him easily to admit that Fisher was correct.⁽²⁸⁾

8.5 Statistics and Agricultural Research: Fisher at Rothamsted

It would of course be grossly misleading to leave an impression of Fisher's work in statistical theory as being solely 'metastatistical' in its nature. In 1919 Fisher was appointed to the newly created post of statistician at/...

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(26) Thus, Yule wrote to M.G. Kendall following the death of Karl Pearson:

I feel as though the Karlovingian era has come to an end, and the Piscatorial era which succeeds it is one in which I can play no part.
(Quoted by M.G. Kendall, 1952, 2)

(27) For example, it incorporated not only Pearson's techniques but also Gosset's. Of course, Fisher's approach was soon to be challenged by others (the E.S. Pearson/J. Neyman theory of inference, and a revitalised Bayesianism) of equal generality and power.

(28) In a letter to Major Greenwood, 26 May 1936, Yule suggested that in the last years of his life Pearson was gradually working his way to a partial acceptance of Fisher's criticisms (Yule Papers, box two).

at the Rothamsted Experimental Station, and some of his most important work was done in the context of the practical demands of agricultural research.

Fisher was not, in fact, the first British statistician to become involved in agricultural research. As pointed out above, the interests of Guinness Brewers included agriculture, as well as brewing. In the period prior to 1914, Gosset was already interested in agricultural research and in contact with workers in England, who, presumably as a result of the resurgence of agricultural research in this period, had already started to apply elementary statistical techniques to the results of agricultural experiments.⁽²⁹⁾ Interestingly enough, it was through these contacts that Gosset first came to know of the work of Fisher, and it may well have been partly through Gosset that Fisher was appointed to Rothamsted.⁽³⁰⁾

Fisher/...

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(29) Wood and Stratton (1910); Mercer and Hall (1911). The revival of agricultural research was presumably due to the recovery from the late-nineteenth century agricultural depression and the start of large-scale state funding, perhaps occasioned in part by the threat of war. In 1902, its new director Daniel Hall found the long-established Rothamsted Experimental Station 'more like a museum than a laboratory' (E.J. Russell, 1966, 233), but under the energetic direction of Hall and his successor, E.J. Russell, it began to revive. The Liberal government set up a £2.5 million development fund for agriculture, and by the outbreak of war in 1914 there were twelve institutes and two minor centres of agricultural research in Britain. (Russell, 1966, 272).

(30) Gosset to Karl Pearson, 12 September 1912, in E.S. Pearson (1968, 406); Gosset to Fisher, 30 December 1918, in McMullen (ed.) (n.d.).

Fisher worked at Rothamsted from 1919 to 1933, and, even after he left to take up the Galton Chair of Eugenics vacated by Karl Pearson, he continued to live in Harpenden and to play an active role in the life of the research station (Yates and Mather, 1963, 94). His publications almost immediately reflected the new environment. Fisher (1921b) shows Fisher getting to grips with the problem that was the immediate cause of his appointment: the existence at Rothamsted of a huge bulk of only partially analysed experimental records (Russell, 1966, 325). In this paper Fisher analysed the wheat yields in 13 plots that had been under continuous observation from 1852 to 1918, developing in the course of the analysis a novel method of curve fitting using orthogonal polynomials. The long series of papers published by Fisher at Rothamsted (reprinted in Bennett, ed., 1971-4, 1 and 2) gives ample evidence of the highly productive nature of Fisher's response to the practical demands of the research station. This response utilised Fisher's previous practical and theoretical statistical experience: an interesting example being the use of the technique of the analysis of variance, originally developed in eugenic research (Fisher, 1918a), as the basis for the design and analysis of agricultural experiments (Fisher and MacKenzie, 1923).

The most important published product of Fisher's early years at Rothamsted was his Statistical Methods for Research Workers (1925). Fisher introduced the book as follows/...

follows:

For several years the author has been working in somewhat intimate co-operation with a number of biological research departments; the present book is in every sense the product of this circumstance. Daily contact with the statistical problems which present themselves to the laboratory worker has stimulated the purely mathematical researches upon which are based the methods here presented. Little experience is sufficient to show that the traditional machinery of statistical processes is wholly unsuited to the needs of practical research. Not only does it take a cannon to shoot a sparrow, but it misses the sparrow! The elaborate mechanism built on the theory of infinitely large samples is not accurate enough for simple laboratory data. Only by systematically tackling small sample problems on their merits does it seem possible to apply accurate tests to practical data.
(1925, vii)

In part, the book was a presentation of Fisher's approach to the foundations of statistical inference together with his extensive work on exact distributions. But it was also more than that. Fisher drew on his experience to show the usefulness of his methods of inference to practical problems. Thus, he showed the applicability of the method of maximum likelihood to the estimation of genetic linkage in self-fertilised animals and plants (1925, 24-5). The Poisson distribution was illustrated with Gosset's work on counting yeast cells (1925, 58-9), and problems of bacterial counting were discussed (1925, 61-4). Chi square was discussed in the context of breeding experiments (1925, 77-90). Gosset's practically-motivated work on small-sample theory was systematically presented and integrated into Fisher's general approach (1925, 101-13). Regression was illustrated by analysis of the effect of nitrogenous fertilisers on grain yield/...

yield and by the comparison of the relative growth rates of two cultures of an alga (1925, 119-25). The discussion of the correlation coefficient showed how Fisher's work on its exact distribution could be used to test the significance of particular values (1925, 138-75). The analysis of variance was presented and illustrated from both genetics and experimental field trials (1925, 188-209). The topic of the analysis of field trials was further developed, and it was shown how the analysis of variance, combined with a restricted but randomised experimental design (the famous 'Latin square') provided a powerful technique for agricultural experimentation (1925, 224-32).

While this approach does not sound exceptional to the modern reader, Statistical Methods for Research Workers was a remarkable innovation. It incorporated Fisher's conviction that a theory of statistical inference could be developed that did not rely on inverse probability and was not restricted to large samples. But, almost more importantly, the book incorporated a new idea for the statistician's role (and therefore a new function for statistical theory). The statistician should get involved in the practical business of experimentation, was the message. This clearly presupposed the diffusion of the type of occupational role that Fisher (and Gosset) occupied. It was not even enough that the scientist should hand his results to the statistician for analysis: experiments (especially large-scale applied experiments that were difficult to 'control') had to be designed/...

designed by those with statistical expertise.

The sales of Fisher's book over the following 25 years indicate something of the diffusion of the model of the role of the statistician and of statistical theory contained in it. 7 editions appeared within 13 years, and by 1950 nearly 20,000 copies in all had been sold (Yates, 1951, 31). Within British statistics, Fisher's work exerted tremendous influence, even amongst those closest to Karl Pearson (E.S. Pearson, 1974). Rothamsted emerged as a centre of statistical research (and even, in an informal sense, teaching, as many came, especially from outside Britain, to learn in an 'apprentice' role) to rival University College. Thus, in 1926, two of Karl Pearson's new staff (Oscar Irwin and John Wishart) left University College 'to gain new experience with R.A. Fisher at Rothamsted' (E.S. Pearson, 1970b, 456).

By the mid-1920's there were, therefore, clear signs of the beginning of a new era in the development of statistical theory in Britain. The new role for the statistician in agricultural and industrial production, and in scientific research in general, may have been in some ways more modest than the position of political influence hoped for by Karl Pearson. The new role was, however, one of considerable importance. Its evolution, and whether the practical demands associated with it can be seen as translating themselves into cognitive interests underlying statistical theory, are interesting problems. They fall, however, outside the scope of this thesis.

Chapter Nine

Conclusion

9.1 Eugenics and the Development of Statistical Theory in Britain

Statistical theory could potentially have developed in response to a wide variety of impulses. Statistical techniques were used in many fields in the period 1865 to 1925. These ranged from administration (official statistics) to astronomy (error theory). From the 1870's to 1914, one particular impulse can, however, be seen as dominant in the institutional and intellectual development of statistical theory in Britain: eugenics. While other factors were of course present, and a unicausal theory of a phenomenon such as the development of statistical theory would be absurd, eugenics seems to have been the most important single factor. Only towards the end of the period covered here, with the work of W.S. Gosset and R.A. Fisher, do other impulses - the needs of industrial and agricultural research and the growth of communal concern for 'metastatistical' problems of consistency and generality - begin to rival eugenics, and only after 1925 do they clearly overtake it.

Francis Galton, Karl Pearson and R.A. Fisher are probably the three most important individuals in the development of statistical theory in Britain in this period. Others such/...

such as Francis Ysidro Edgeworth and George Udny Yule were by no means their intellectual inferiors, but left nothing like the same historical mark. Galton provided the basic tools of correlation and regression that were the chief intellectual stock-in-trade of British statistical theory up to 1914. By personal contact and the exemplars provided by his work, he was also responsible for the recruitment of many of those who started work in the field before 1900. It is clear that the major impulse behind Galton's statistical work was his enthusiasm for eugenics. Not all those that he recruited were similarly motivated; but it would seem from the material reviewed in chapter five that his contacts with those who were not attracted by his overall programme tended to be shortlived and relatively unfruitful. Only two mathematicians seem to have been fired with enthusiasm for the notion of quantifying the theory of heredity and evolution. One, Arthur Black, died before the promise shown by his early work could develop. The other, Karl Pearson, was the man who succeeded in taking over where Galton had left off. He took Galton's brilliant but mathematically crude insights, and refined and systematised them. With Galton's moral and financial assistance, he created in the Biometric and Eugenic Laboratories, and later in the Department of Applied Statistics at University College, the teaching and research core of the new discipline of statistical theory. In Biometrika, he (along with Galton and Pearson's zoologist colleague W.F.R. Weldon) gave the new discipline its major organ of communication and dissemination/...

semination of knowledge.

In the case of Pearson, eugenics was the increasingly pivotal aspect of an interrelated network of intellectual concerns: meritocratic socialism, a social Darwinism orientated to the struggle between nations, a reformist feminism, a naturalistic ethical theory and a positivist epistemology. Evolution was crucial to Pearson. By quantifying the theory of evolution and making it scientific according to his positivist standards, he felt he could produce a science not just of biology, but of society. It was a science that could be (and was) used to refute anti-socialist arguments, to investigate the claims of feminism, and, above all, to show how nations could be made fit for the struggle for existence. Pearson's quantitative social Darwinism was in some ways a broader enterprise than Galton's eugenics, but eugenics was still at its core. Human evolution was studied so as to make intervention in the process, and ultimately conscious control over it, possible. Pearson's statistical theory developed as the intellectual foundation of this positivist evolutionism, as a series of 'Mathematical Contributions to the Theory of Evolution'.

Pearson's intellectual work and his research institute attracted many students, an important minority of whom went on to become important figures in twentieth century British statistical theory. At least some of those who studied at University College came because they found Pearson's/...

Pearson's overall intellectual programme attractive. Even those who came simply to obtain a unique training in statistical theory came, after all, to a research institute and department which were made possible largely by money for eugenics. The close ties between eugenics and statistics can be seen in the fact that Pearson, as Head of the Department of Applied Statistics, bore the title Galton Professor of Eugenics.

R.A. Fisher, the third of the three crucial figures of this period, never in fact studied at University College. Eugenics, however, seems to have played an important role in his beginning work on statistical theory. Fisher was a co-founder of the Cambridge University Eugenics Society, and eugenics was a major inspiration of his early work, especially in statistical biology. Further, even if Fisher eventually pushed statistical theory in a different direction from that followed by Pearson and Galton, he did not do so ab nihilo, but by building on earlier, in part eugenically-inspired, developments.

Comparison with other countries is useful to set the British experience in context.⁽¹⁾ This intimate connection/...

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- (1) The available material is very patchy. Koren (ed.) (1970) has an international perspective, but deals primarily with official statistics, not statistical theory: for the former, Westergaard (1932) remains the best source for the period up to 1900. There are a number of useful works referring to specific countries. For France, see T.N. Clark (1967; 1973, especially 122-46). For Italy, see Gini (1926). For Germany, see Lexis (1893) and Oberschall (1965). For Russia, see Zarkovich (1956; 1962), Maistrov (1974, 161-224) and Adams (1974, 69-98). For Scandinavia, see Särndal (1971). For the United States, see Owen (1976) and Ben-David (1971, 149-50).

nection between statistics and eugenics appears to have been a phenomenon unique to Britain. In other countries, statistical theory remained tied to older concerns such as administrative and social statistics (especially in France and Italy) and error theory (especially in Scandinavia). The use of statistics in agricultural research did become of importance in the Soviet Union and the United States, but only towards the end of the period considered here.

It would be very difficult to reach a measured judgment on the relative importance to statistical theory of the different national contributions of this period. To someone trained in the British tradition of statistical theory, the British contribution naturally seems paramount, because so much of what one is now taught as statistical theory (regression, correlation, the chi square test, t test, analysis of variance, method of maximum likelihood, and so on) can be traced in large part to the British work of this period. But this could well be a point of view informed by an overly ethnocentric notion of what is salient in modern statistical theory. The histories of the non-British schools of statistics (especially the Russian) are, in any case, as yet largely unwritten.

It does, however, seem likely that, whatever judgment may ultimately be arrived at as to the relative importance of different national contributions, the content of these contributions will be seen as significantly different. It may well be that these differences can be accounted for by the/...

the differing contexts in which statistical theory developed. Thus, in Italy a school of statistical theory emerged, approximately contemporaneous with the British school, but institutionalised in a very different context and producing a different kind of statistical theory. According to Corrado Gini (1884-1965), the leading twentieth century Italian statistician, the Italian school was based on a conscious opposition to a single-minded focus on mathematical approaches to statistics (Gini, 1926, 707). Instead of building a powerful mathematical apparatus based on somewhat narrow assumptions (for example, the assumption of normally-distributed populations), the Italian school developed a wide variety of descriptive measures for use in a similarly wide range of applications (the Gini coefficient of income inequality is the best known to British statisticians). This tendency can perhaps be accounted for by the fact that statistics developed as an academic discipline in Italy within law faculties, and most job opportunities lay in the state statistical service (Gini, 1926, 704-6). Usefulness in diverse social, economic and administrative studies was therefore at a premium; mathematical sophistication at a discount. A movement calling itself eugenist did develop in Italy, in which Gini and some other statisticians played a major role, but, perhaps in accordance with the specific ideology of Italian fascism, its concerns were with broad demographic changes. Studies of heredity and social class were not prominent, and may even have been discouraged by a corporatist/...

corporatist ideology that played down class differences. The Italian eugenics movement does not, therefore, seem to have shifted the attention of Italian statisticians from their traditional area of study to the type of concerns which exercised British statisticians.⁽²⁾

The dominant tradition in British statistics was connected only weakly with international developments. Egon Pearson (1967, 340-1) comments on the lack of acquaintance of British statisticians of this period with much of the continental literature. Only Edgeworth, whose work followed a quite different direction from that of Galton and Pearson, appears to have made much use of the statistical theorising of contemporaries such as Lexis. This is, of course, hardly surprising. Statistical theory was only beginning to emerge into its modern form of a developed international discipline. Men such as Galton, Pearson and Arthur Black saw themselves not as contributing to an existing well-developed field, but as pioneering a new one: the statistical study of heredity and evolution. Historically, it is in this context that their work must be seen.

Thus, it would appear that in studying British statistical theory in this period we are dealing with a differentiated/...

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- (2) An unpublished paper on the Italian eugenics movement by George S. MacPherson of the Department of the History and Sociology of Science, University of Pennsylvania, is, to my knowledge, the only specific secondary source on Italian eugenics (MacPherson, 1973). For C. Gini, see his papers (1927; 1930), also T. Salvemini (1968).

ferentiated tradition, relatively separate from other national schools, and developing statistical theory in its own distinctive way. A comparative perspective suggests, therefore, that it is plausible that much of what was specific to Britain in the development of statistical theory can be accounted for by the specifically British connection between statistics and eugenics.⁽³⁾

9.2 Knowledge and Interests

This study confirms the claim by Ben-David (1971) that social factors external to science can affect the rate of scientific advance. If we take statistical theory as 'internal' to science and the eugenics movement as 'external' to it, then it is clear that the latter 'external' factor did indeed affect the pace with which the former scientific field developed.⁽⁴⁾ On the other hand, this study disconfirms/...

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- (3) To attempt to answer the further question as to why this connection was specifically British would be, in the existing state of comparative knowledge, a purely speculative exercise. Part of the answer may, however, lie in the fact that Britain, as well as being the home of eugenics, was also the home of Darwinism, with its implicitly statistical concept of the species. When Galton and Pearson turned to eugenics they did so with Darwinian, and thus statistical, eyes.
- (4) It must, however, be noted that this study also shows the problematic nature of the 'internal'/'external' boundary. Most of the major figures discussed here would have argued that eugenics was part of science, or that eugenics was an application of scientific knowledge and not something 'external' to science.

confirms Ben-David's view that social factors are largely irrelevant to the conceptual growth of science. The political and ideological goals of the eugenics movement, which were, as has been argued in chapter three, themselves an expression of the social interests of the professional middle class, affected the content of innovations in statistical theory. From Galton's work in the 1870's on reversion to that of Pearson in the early 1900's on association, the conceptual development of statistics was markedly affected by eugenics. Similarly an 'external' factor - the needs of production in the brewing industry - formed the impulse that led Gosset to his crucial theoretical break with the dominant tradition of statistical theory. Social goals 'external' to statistical theory - and indeed 'external' to science as we now understand it - played a constitutive role in statistical innovation.

The possibility exists, however, that social influence on innovation might have been of only marginal importance. A priori, it is not implausible that needs connected with particular applications generated theoretical innovations but that these innovations were then judged according to perfectly general criteria in no way related to these needs. If assessment were context-independent, then social factors conditioning innovation would be at least partially cancelled out in their effects, for only those innovations of genuine worth would be judged valid.

This/...

This radical separation of the generation of innovation and its assessment does not seem justifiable in the light of the material discussed in the preceding chapters. In such cases as Galton's assessment of error theory, Bateson's or Fisher's assessment of evolutionary theory, Pearson's assessment of the work of Yule, or Gosset's assessment of biometric statistical theory, there seems to be no great difference between the factors conditioning assessment and those conditioning the individual's own innovative work. Assessment, like innovation, was context-bound and structured by goals and interests of specific kinds. Nor was there a disinterested community of statisticians to whose context-free judgments we can turn. The dominant tradition in statistical theory, the biometric school, manifested an evaluative orientation which was, as has been shown in chapters six and seven, closely bound to specific interests.

It would of course be possible to conclude that such context-bound and interest-related assessments should be discounted as inadequate, unscientific and biased. This material would then be taken as an instance of 'external factors' producing bad science, and the episodes discussed here would be seen as temporary set-backs on the road to properly scientific statistical theory. Such an interpretation cannot be straightforwardly refuted, but it does have to face certain problems. It would have to be concluded that throughout a prolonged period of activity that no-one/...

no-one would deny was crucial to the development of statistical theory, judgment has been revealed to be typically less than 'properly scientific'. Further, it would appear to be extraordinarily difficult to specify on what basis 'properly scientific' judgments could actually have been made. It is easy on the basis of present-day knowledge to classify judgments as 'right' or 'wrong': Galton was right about regression; Pearson was wrong about association; Fisher was right about evolution; Bateson was wrong about evolution; Gosset was right about small samples; and so on. But such classification cannot easily be related to the actual historical basis of judgments. Very limited specific interests sometimes produced 'right' judgments, as in the case of Gosset. The most sophisticated and methodologically aware thinker considered here, Karl Pearson, was perhaps most often 'wrong'. The same interests led both to 'right' judgments (Galton on regression) and to 'wrong' judgments (Pearson on association).

Thus, at best, the 'properly scientific judgment' becomes a most mysterious entity: rare in practice, historically unimportant in the construction of our present knowledge, and, apparently, no more likely to lead to correct conclusions than narrowly interested judgments. Few of the scientists discussed here would have admitted that their judgments were less than 'properly scientific': it is paradoxical that Gosset, who was perhaps the only one who would happily have agreed that his judgments were structured by/...

by narrow interests, would now be seen as 'right'. It becomes increasingly difficult, in the light of considerations such as these, to believe that the notion of an abstract 'properly scientific judgment' can be of historical use. To dismiss the context-bound and interest-laden judgments of these scientists as ipso facto inadequate seems facile and gratuitous. Surely it is better to seek an alternative, less dismissive, approach.

Fundamentally, this approach must consist of seeing context-bound and interest-laden judgments as neither necessarily wrong nor necessarily inadequate, but simply as judgments. Let us admit that no actual scientific judgment can be envisaged that compares a knowledge-claim with the whole universe or tests a technique in all its possible applications, and conclude that all judgments must in some way be 'limited'. Our efforts can then be devoted to developing a framework to help us understand scientific judgments in their contexts and in their relations to interests.

In the introduction to this thesis it was suggested that a useful pointer to such a framework might lie in the work of Habermas (1972). The notion of knowledge-constitutive cognitive interests has been, I would argue, helpful in understanding the concrete materials presented here. The notion does not artificially separate 'discovery' and 'justification', nor does it carry any pejorative connotations. Can we then see Habermas's overall approach as valid/...

valid here? Habermas suggests that constitutive of judgment in the 'empirical-analytical sciences' is a frame of reference that defines both the object of these sciences ('objectified processes') and their goal (an increase in the ability to predict and control natural processes, as exemplified by successful experimentation or useful technology). A cognitive interest in prediction and control is thus constitutive of those disciplines which fall under the rubric of the natural sciences.

There is much to suggest the plausibility of analysing the statistical theory studied here as embodying an interest in prediction and control of objectified processes. Objectification is arguably intrinsic to the basic statistical procedures of measuring and classification (see, for example, Cicourel, 1964, 7-38). Statistical inference was (and is) an attempt to predict from a known sample the characteristics of an unknown population. A regression analysis yields a rule for predicting the expected value of one variable from that of another variable or set of variables. Successful statistical procedures enhance the potential for control; this is most obviously the case when statistical theory is used in production ('quality control', yield trials, and so on), but it holds also in other areas (for example, in the relationship between biometric statistical theory and eugenic intervention).

Despite these useful insights to be gained from Habermas's/...

Habermas's approach, it cannot be accepted without qualification. My first reservation concerns the separation of instrumental action and meaningful communication to be found, for example, in Habermas's distinction between the interests constitutive of the 'empirical-analytic' and 'hermeneutic' sciences. This separation appears to be too rigid. To forget that scientific prediction and control takes place within a shared framework of meanings and assumptions, which is sustained by consensus and authority, would be to neglect completely the insights of the Kuhnian and post-Kuhnian history and philosophy of science (see Barnes, 1977, 17-18). To see Pearson's eugenic statistics as simply a form of objectified prediction and control, and not also as a contribution to ideological communication about social values, would be to ignore its historical and biographical context. It seems desirable to withdraw from the separation of instrumental action and communication to a concept that includes both: perhaps the Marxist notion of 'practice'. To relate Pearson's statistics to eugenics as a possible 'practice' means to relate it to an activity that had, if it was to be successful, to involve persuading (or bullying) people into compliance, as well as predicting and controlling the characteristics of human populations. The eugenisists had to communicate as well as to control.

My first reservation is thus that the interests informing science must be seen as wider than those of extending the scope of instrumental action. My second reservation is/...

is that these interests are differentiated to a greater degree than Habermas might suggest. If scientific knowledge were indeed informed by a single, unitary interest in prediction and control, we would return, by a circuitous route, to a context-independent criterion for scientific judgments. Admittedly, this criterion would be interest-laden, but the interest concerned could justifiably be claimed to be a general human interest. There is, however, little in this study to lend plausibility to an 'abstracted instrumentalism' of this kind. Thus, in the controversies discussed here, both sides might quite reasonably have claimed to be extending the scope of prediction and control. Yet the ways in which they were doing so were radically different, and it is extremely difficult to imagine a wholly neutral way of testing which side was making the greater contributions to the furtherance of prediction and control. Instead, it must apparently be concluded that the cognitive interests informing the work of the scientists considered here were never fully general interests in prediction and control, but always interests that were 'situated', that were made concrete with reference to specific exemplars and to particular forms of prediction and control of particular processes.

Having admitted this, we can then ask the question: what caused the actual observed particularisations of interests, or selection of criteria for judgment, manifested in the work of given participants? The answer to this question/...

question seems typically to involve matters of cultural context (such as availability of exemplars of successful prediction) or of social interest. The social interests involved appear to have been sometimes narrow, esoteric and 'internal' to science (such as those of professional biologists in preserving the value of their competences), and sometimes general, political and 'external' to science (such as those of the professional middle class as mediated through the eugenics movement).

The view suggested here is thus one in which scientific knowledge is constitutively linked to practice through situated cognitive interests. It is to be hoped that the concrete materials presented here indicate at least the plausibility of this view in this particular instance. To discuss fully the general implications of this view is outside the scope of this work. One crucial point, however, must be raised: the relationship of this historical analysis to questions of the status of present-day knowledge in statistical theory.

To say, following Habermas, that interests are constitutive of knowledge is to invite a possible misunderstanding. The German language differentiates between two aspects of the notion of 'knowledge': Erkenntnis ('the act, process, form or faculty of knowing') and Wissen ('the passive content of what is known'). Habermas's analysis refers to the first, rather than the second (Habermas, 1972, 319). So must any similar analysis, if it is to avoid the 'genetic/...

'genetic fallacy' of concluding that the origins of knowledge forever determine its status. Knowledge must be analysed as a resource for practice, and knowing must be seen as a process. Only in this way can the 'genetic fallacy' be avoided.⁽⁵⁾

The analogy between knowledge as a resource for practice and tools in the everyday sense may make this point clearer. A tool's construction will reflect the tasks for which it was designed, and it will initially be evaluated according to its adequacy in the performance of these tasks. This does not mean, however, that its use is always limited to these tasks; it may well be found helpful for purposes quite different from those for which it was developed. Similarly, the construction and evaluation of knowledge can be structured by cognitive interests without these determining for all time the fate of this knowledge. Of course, it is true that the initial uses of a tool may well give us a clue as to other possible uses, may suggest the amendments that will be required to achieve different purposes with it, and/...

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- (5) In other words, the notion of knowledge-constitutive cognitive interests makes sense only if knowledge is treated as a resource for practice and not, as in mathematical realism, as a collection of objects. In the light of evidence showing the context-bound and interest-laden nature of scientific evaluation, the realist must presumably argue for the irrelevance of the adequacy of the procedures of evaluation to the question of the truth of what is being evaluated. This, however, leaves the issue of how we know the content of the 'real' highly problematic.

and may indicate in which situations we may have to discard it. All of this, however, is contingent, not necessary.

That Galton's and Pearson's eugenic concerns structured their statistical theory does not imply, therefore, that the modern statistician who does not share these concerns need necessarily eschew the use of the concepts developed by them. It is not that the acceptance of a technique by modern statisticians guarantees its context-independent and interest-free validity. Rather, the construction and evaluation of statistical theory by modern statisticians needs to be studied in its own right before any conclusions can be drawn as to the cognitive interests constitutive of present-day statistics.

'Our statistics is different', the modern statistician may well claim. To say this is false in one sense, true in another. It is false, in that to claim that 'we' have achieved eternally valid knowledge, or evaluations not structured by context or interest, would be unjustifiable. It is true, to the extent that 'our' statistical theory has emerged in a historical process from 'theirs'. This historical process has largely been one of the generalisation of the scope of statistical theory, as statisticians have come to grips with new situations. 'Their' concepts have been modified, stretched or discarded. So 'our' statistics is in this sense more general than 'theirs', and hence it is relatively easy for us to see the context-bound nature of 'their' thought. It is not that 'our' statistics explains/...

explains 'theirs' as a special case; rather, 'theirs' helps to explain 'ours', in that 'their' knowledge was used in the construction of 'ours'. It is not, as a realist might have it, that Galton and Pearson discovered some of the current stock of truths; rather, it is that they, in solving their problems, produced resources that have been used by later statisticians to solve other problems. Like 'theirs', 'our' statistics is a social and historical product and can be studied by sociological and historical methods; but it is different from 'theirs' in that it has evolved from it.

9.3 Patterns of Explanation

The analysis of patterns of cognitive interest can be carried out largely with the published and manuscript sources that are used in any intellectual history. An account is produced of the development of an area of knowledge that can be compared with other accounts, and thus checked. However, in moving from the analysis of cognitive interests to relating those cognitive interests to social interests, new problems are necessarily encountered. If these latter interests are not 'disciplinary', but wider social interests, we immediately enter the domain of general history and sociology.

The sociology of knowledge hypotheses presented in this study can rest only in small part on the documentary evidence used here. Even the material on the social composition/...

position and programme of the eugenics movement presented in chapter three is far from sufficient. To support these hypotheses properly would require a general theory of social structure and social interests applied to the situation of intellectuals in late Victorian and Edwardian Britain. The necessary historical and sociological work has hardly begun. Thus, the hypotheses put forward here remain unconfirmed conjectures. The most I can hope to have done is to have indicated their plausibility and potential fruitfulness for further research, and perhaps to have thrown some light on the methodological problems involved in applying the sociology of knowledge to a scientific specialty such as statistical theory.

One difficulty is that the direction of the sociological explanation used here is opposite to the direction of the historical investigation. The historical investigation began with the documentary evidence and worked outwards into the surrounding society. The sociological explanation, however, begins with the structure of that society and the interests of various classes in it, and goes on to hypothesise how these interests might have led to the transformation of existing scientific culture and to the production of new knowledge. These two processes of study are of course only formally separate: I have tried as far as I am able to follow Lucien Goldmann's injunction to integrate them dialectically, to move from the text to the society and back to the text, and so on (Goldmann, 1964). But/...

But there remains a persistent problem of presentation. The object of this study is a given body of knowledge produced by a historically unique group of individuals. The evidence used is a set of particular writings and individual biographies. The explanation used is, however, a structural one, which does not give a deterministic account of individual behaviour. It does not, for example, claim that if Karl Pearson's brother had become a statistician he would have developed statistical theory in the way Karl Pearson did. Thus, the sociological account put forward here, even if it were to be verified by further studies, in no sense yields a necessary and sufficient explanation of the specific object of this study.

In essence, this is a simple point. As Marx put it, people

... make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given and transmitted from the past.
(1968, 97)

For the freedom of people to make their own history, we can here read their freedom to produce their own knowledge. A strict individual, or even a statistical, determinism is therefore not to be expected. On the other hand, individual creativity must operate in a given historical situation. The resources for innovative thought to be found in a given cultural tradition and social setting are limited. In a divided society, knowledge (especially knowledge of such topics/...

topics as human heredity and social evolution) is used in the construction of legitimations and ideologies. Thus, Karl Pearson, say, did not start from nothing: his thought developed within in a given cultural context, where ideas of heredity and evolution already had their social uses. Further, he had no choice as to the family into which he was born; he had a choice of how to lead his life, but not an unrestricted one, as he had at least in part to choose between given options. These factors are by no means sufficient to determine the course of his life and thought. They do, however, point to the fact that as an individual he had to act in a given historical situation. In the limited sphere of culture in which he operated, he was able to transform that situation: but even that he could do only with given materials.

So an individual such as Karl Pearson can be regarded as a 'trace element'. Pearson's work constitutes an important and clear contribution to the construction of eugenic ideology as an appropriate expression of the social interests of the professional middle class. By examining it, we can perceive some aspects of the general character of the connections between ideas and interests at the time. Further, social interests do not affect ideas in a disembodied way, but through the concrete practice, thinking and writing of individuals and groups of individuals. The study of Pearson and his followers thus reveals to us one of the more important routes whereby interests influenced ideas/...

ideas.

It is perhaps worth ending by restating, in the light of the above provisos, the overall structural argument put forward in the preceding chapters. It is suggested that during the latter part of the nineteenth century the social stratum composed of scientifically-based professional occupations gradually developed at least a limited degree of self-awareness. Previously, this stratum had tended to provide intellectual spokesmen for other, more fundamental social classes: the aristocracy, bourgeoisie and, to a much lesser extent, the working class.⁽⁶⁾ This situation gradually changed, presumably as a result of the growing numerical and social weight of this stratum. The first signs of the change were in its interlinked *élite*, the 'intellectual aristocracy'; towards the end of the century indications of the change appear in the '*nouvelle couche sociale*' of professional employees (Hobsbawm, 1968). Ideologies were elaborated which celebrated the professional stratum itself. It was claimed that the knowledge, skills and 'mental ability' of the professional expert were of paramount importance in a modernising society, and that these deserved respect and rewards equal to or greater than those owing to title, wealth or manual work. Eugenics is taken here as instance of one of these ideologies (chapter three). Galton, Pearson and Fisher are taken as individuals making important contributions to this emerging professional/...

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(6) For documentation of this see Perkin (1972, 218-70).

professional ideology. Galton is taken as a member of the 'intellectual aristocracy'; Pearson and Fisher as members of the 'nouvelle couche sociale'. The forms in which they developed this ideology differed: for example, Pearson constructed a left-wing variant while Galton and Fisher constructed relatively right-wing variants. Nonetheless, in all three cases, the same basic social interests were manifested in the system of belief constructed.

The work of those, such as Bateson and Yule, who stood aside from or opposed these developments can be seen, I would tentatively suggest, as expressing different social interests. The process of 'modernisation' inevitably caused a deterioration in the relative position of some old élite groups. Fragments of these may have reacted, not by seeking an accommodation with the new order, but by opposing it, or by seeking to insulate themselves from it. Pearson's positivist and collectivist version of eugenics might well be expected to be particularly distasteful to those in this position.

I would thus conjecture that it is useful to see at least two distinct constellations of interests as manifested in the thought of the British intelligentsia of the Victorian and Edwardian period. One was grounded in the situation of those professional occupations which were growing in importance with modernisation; it found expression in technocratic ideologies such as Fabianism and eugenics. The/...

The other was grounded in the situation of those disparate members of the old élite (such as downwardly-mobile offspring) to whom modernisation posed a threat; this constellation of interests found expression in various forms of conservatism, but not in scientific ideologies such as eugenics. This remains only a conjecture. Given such factors as the contingency of individual biographies and the crosscutting effects of some particular occupational affiliations, I would not expect clear and straightforward patterns to emerge from future studies. Nonetheless, I would advocate its use as a hypothesis that, though perhaps in a modified form, may eventually throw light on some aspects of the history of science, and of intellectual life in general, in this period.

Appendix A

Archival Sources

The following archival sources were consulted in the course of the preparation of this thesis, and I should like to express my thanks to the relevant individuals and institutions for permission to see them. I was unable to find or obtain access to two sets of papers. The first of these consists of the technical reports, etc., prepared by W.S. Gosset while employed by the Guinness Brewery in Dublin. These were used by E.S. Pearson in writing his biography of Gosset (E.S. Pearson, 1939), and thus this omission is perhaps not too serious.⁽¹⁾ Access to the papers of R.A. Fisher, in the care of the Department of Genetics, University of Adelaide, was refused. Much of what is written in this thesis concerning Fisher must therefore be liable to correction or addition once the Fisher Papers are opened. In addition, I was unable to obtain access to the papers of Karl Pearson until after the first draft of this thesis was virtually complete, and thus I have not been able to make as full use of them as I would have liked.

Bateson/...

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(1) I should like to thank Mr. A.V. Vincent, Head of Management Services, Arthur Guinness Son & Co. (Dublin) Ltd., who attempted to locate these for me.

Bateson Papers.

Dr Alan Cock kindly allowed me to see parts of his copy of a microfilm of the papers of William Bateson prepared by William Coleman. Sections 10a-c, 13, 14b, 15 and 18 contain material relevant to Bateson's controversy with the biometricians. (In addition, Professor C.D. Darlington showed me some further correspondence of Bateson's not on the microfilm, as did the Librarian of St. John's College, Cambridge.)

Black's Notebooks.

A set of 23 manuscript notebooks by Arthur Black. These were found for me by Messrs David and Richard Garnett, Mr David Garnett has kindly allowed them to be placed in the Library of University College, London.

Cambridge University Eugenics Society Papers.

These were found in the library of the Eugenics Society, Eccleston Square, London SW1, under reference C.1.393. They consist of a set of manuscripts, typescripts and press-cuttings referring to the activities of the Cambridge University Eugenics Society, and contain previously unknown papers by R.A. Fisher (1911; 1912a; 1912b).

Darwin, Leonard Papers.

Dr Roy MacLeod kindly allowed me to examine a set of the papers of Major Leonard Darwin being catalogued at the University of Sussex and now in Cambridge University Library. Unfortunately, they provided no information on the chief point of interest, the relations between Major Darwin and R.A. Fisher/...

R.A. Fisher. The few items of correspondence of Major Darwin's in the care of the Royal College of Surgeons at Down House include no letters to or from Fisher.

Davenport Papers.

These are in the care of the American Philosophical Society, Philadelphia. They include letters from Francis Galton, Karl Pearson, R.A. Fisher. Although they are of considerable general interest, the Davenport papers do not in general throw much light on the development of statistical theory.

Galton Papers.

These are in the Library of University College, London. Along with the Pearson Papers (and possibly the Fisher Papers when they are opened), they form the major archival source on the history of statistical theory in Britain. A handlist compiled by M. Merrington and J. Golden was issued in 1976.

Pearl Papers.

These are in the care of the American Philosophical Society, Philadelphia. From the point of view of British statistics, the most interesting part of these papers is the extensive correspondence from Major Greenwood and George Udny Yule, which contains a lot of informal information on the British statistical community.

Pearson Papers.

These are now in the Library of University College, London. Although a handlist compiled by M. Merrington was issued in 1974/...

1974, they were not opened to scholars in general until 1977. Material of particular interest includes Pearson's first written reflections on Galton's work (Pearson, 1889) and his correspondence with colleagues such as Yule.

Royal Statistical Society Minutes.

The minutes of the Council and Executive Committee of the Society for the period of the thesis were examined. In the light of their formal nature (and of the relatively small role of the Society in the development of statistical theory in this period), these proved to be of little interest. The records of the Society for the period after 1930 (such as material on the 'Study Group', on the Industrial and Agricultural Research Section and on the activities of the Society in the Second World War) are of greater interest, but fall outside the scope of this thesis.

Yule-Greenwood Letters.

These consist of a set of letters from George Udny Yule to Major Greenwood, arranged in chronological order and dating from the period 1910 to 1949, in the possession of Mr. George B. Greenwood, 2, Burhill Road, Hersham, Walton-on-Thames, Surrey. This series of letters, which appears reasonably complete, is probably the best single manuscript source for the study of Yule.

Yule's Notes.

These are five manuscript notebooks by George Udny Yule, and are his notes of Karl Pearson's lectures on statistical theory/...

theory in the academic years 1894-5 and 1895-6. They were given by Yule to the Department of Statistics, University College, London. These proved particularly useful in elucidating some otherwise opaque published work (in particular, Pearson and Filon, 1898). They are now in the Pearson Papers (84).

Yule Papers.

These are the papers of George Udny Yule in the care of the Royal Statistical Society. Of particular interest are letters between Yule and Major Greenwood prior to 1914, including letters from Greenwood to Yule not duplicated in the Yule-Greenwood Letters, and material gathered by Yule in writing his obituary of Karl Pearson.

| | | |
|-------------------|-----------------|-----------|
| Clergy | 3 | 3 |
| Social Workers | 3 | 2 |
| Scientists | 2 | 10 |
| Artists | 2 | 2 |
| Military Officers | 3 | 4 |
| Teachers | 3 | 1 |
| Journalists | 2 | 2 |
| Not Known | 48 ⁿ | 0 |
| Totals | 111 | 33 |

1. Includes five who had the title 'Dr' but about whom no further information was available.
2. Includes Col. H.B. Miller, DSO, who was a military officer specializing in military engineering.
3. Includes Havelock Ellis whose writings were largely scientific.
4. Includes eight people who had university degrees and one with the title, 'Sir' or 'Lord'.
5. The well-documented number refers to those for whom reliable biographical information was available.

Appendix B

The Social Composition of the Eugenics
Education Society

Farrall's data refer mainly to two groups: the members of the Council from 1908-20, and a random sample of 60 members and associate members of the Society from 1912-13. With his kind permission, I reproduce his data:

Occupations of the Members of the EES Council

| <u>Occupation</u> | <u>Total</u> | <u>Well-documented number</u> |
|-------------------|-----------------|-----------------------------------|
| Medical | 26 ^a | 10 |
| Academic | 18 | 16 |
| Politicians | 4 | 3 |
| Clergy | 3 | 3 |
| Social Work | 3 | 3 |
| Scientists | 2 | 2 ^b |
| Writers | 2 | 2 ^c |
| Military Officers | 2 | 1 |
| Lawyers | 1 | 1 |
| Housewives | 2 | 2 |
| Not Known | 48 ^d | 0 |
| Totals | 111 | 43 |

- a. Includes five who had the title 'Dr' but about whom no further information was available.
- b. Includes Col. H.E. Hills, FRS, who was a military officer specializing in military engineering.
- c. Includes Havelock Ellis whose writings were largely scientific.
- d. Includes eight people who had university degrees and ten with the title, 'Sir' or 'Lady'.

The 'well-documented number' refers to those for whom definite biographical information was available.

Source: Farrall (1970, 221)

Occupations of the Members of the Random Sample

| <u>Occupation</u> | <u>Number</u> | <u>Occupation</u> | <u>Number</u> |
|----------------------|---------------|-------------------------------|---------------|
| Academic | 6 | Wife ^a | 5 |
| Medical | 3 | Lawyer | 1 |
| Social Work | 2 | Director of Art Museum | 1 |
| Writer | 2 | Local Government | 1 |
| Clergy | 1 | Part-time author ^b | 2 |
| Military Officer | 1 | No Information | 35 |
| <hr/> Total 60 <hr/> | | | |

a. All were wives of prominent people.

b. These two members are known only because of the one or two books they each wrote

Source: Farrall (1970, 227)

The group of 41 Council members for 1914 is a subset of Farrall's group of 111 Council members for 1908-20, and can be seen as a check on the 'not known' or not 'well-documented' cases in Farrall's list. We see that there is in fact no reason to doubt his conclusions. Individuals already identified by Farrall are asterisked.

*President: Major Leonard Darwin, Son of Charles Darwin. Retired army engineer.
(Who was Who, 1929-40.)

*Hon. Secretary: Mrs. Sybil Gotto, Hon. Secretary 1907-20. Widow of Naval Officer. Effectively worked full-time for eugenics. (Eugenics Review, 47 [1955-6], 149.)

Hon. Treasurer: Paul von Fleischl. Treasurer of E.E.S., 1907-22. Occupation unknown.

Mr Crofton Black: Barrister and official of Land

Union. (E.E.S., Sixth Annual Report, 25, and Eugenics Review, 12 [1920-21], 91.)

Sir Edward Brabrook: Barrister, Chief Registrar of Friendly Societies, 1891-1904. Director of Society of Antiquaries and former President of the Anthropological Institute. (Who's Who, 1914.)

Mrs Theodore Chambers: Wife of Theodore Chambers, civil servant and businessman. (Who was Who, 1951-60.)

Hon. Sir John Cockburn: Former Minister of Education, South Australia. Doctor. Represented Australia at international conferences on health, eugenics, etc. (Who was Who, 1914.)

Mr R. Newton Crane: International lawyer. (Who's Who, 1914.)

Mr A.E. Crawley: Author. Wrote on anthropology, sport, etc. (Who's Who, 1914.)

Sir H. Cunningham: Former lawyer and judge in India. (Who's Who, 1914.)

Dr Langdon Down: Physician to National Association for Welfare of Feeble-Minded. (Medical Directory, 1914.)

- *Mr. Havelock Ellis: Scientist and author. (Who's Who, 1914.)
- Prof. J. Findlay: Professor of Education, University of Manchester. (Who's Who, 1914.)
- Mr E.G. Wheler Galton: Nephew of Francis Galton. Farmer at Claverdon. Interested in scientific aspects of agriculture. (K. Pearson, 1914-30.)
- *Dr. M. Greenwood: Medical statistician. See chapter five.
- Dr. W. Hadley Lecturer in Medicine, London Hospital. Physician, Chest Hospital, Victoria Park. (Who's Who, 1914.)
- Mrs W.H. Henderson: Wife of Admiral Henderson, who since retirement had served on Metropolitan Asylums Board. (Who's Who, 1914.)
- *Major E.H. Hills, F.R.S.: Director of Durham University, Observatory, President of Royal Astronomical Society. Former military engineer. (Who's Who, 1914.)
- Very Rev.W.R. Inge: Dean of St. Paul's. Former Professor of Divinity at Cambridge. (Who's Who, 1914.)
- Miss Kirby: Secretary of National Association for Welfare of Feeble-Minded. (Eugenics Review, 1 [1909-10] , 85.)

- Dr Ernest Lane: Senior surgeon, St. Mary's Hospital.
(Who's Who, 1914.)
- *Prof.E.W. Macbride: Professor of Zoology, Imperial College.
(Who's Who, 1914.)
- Lady Owen MacKenzie: Widow of Sir George Sutherland
MacKenzie (1844-1910), merchant and
geographer. (D.N.B.)
- *Mr Robert Mond: Industrial Chemist, Director of
Brunner, Mond & Co. (Who's Who, 1914.)
- *Dr F.W. Mott, FRS: Neuropathologist. Physician to
Charing Cross Hospital (Who's Who, 1914.)
- Mr G.P. Mudge: Surgeon, university teacher, and
author of biology textbooks.
(University of London Calendar and
British Museum Catalogue.)
- *Mrs G. Pooley: Wife of opthalmic surgeon, G.H. Pooley.
(Who's Who, 1914.)
- *Mr W. Rae, M.P.: Liberal M.P. for Scarborough. (Who's
Who, 1914.)
- *Dr Archdall Reid: Physician and author of books on
heredity, alcoholism, etc. (Medical
Directory, 1914.)
- Mr John Russell: Headmaster of King Alfred's School,
Hampstead. (Alumni Cantabrigienses,
Part II.)

- *Mr F.C.S. Schiller: Philosopher, Oxford University.
(Who's Who, 1914.)
- *Prof. A. Schuster, Secretary of Royal Society. Formerly Professor of Physics, University of Manchester. (Who's Who, 1914.)
F.R.S.:
- *Mr Edgar Schuster: Former Galton research fellow in eugenics. In 1914 at Oxford University. (Paton and Phillips, 1973.)
- *Dr C.G. Seligmann: Professor of Ethnology, University of London. Formerly Hunterian Professor at Royal College of Surgeons. (Who's Who, 1914.)
- *Prof. C. Spearman: Grote Professor of Mind and Logic, University of London. (Who's Who, 1914.)
- *Prof. J.A. Thomson: Professor of Natural History, University of Aberdeen. (Who's Who, 1914.)
- Dr A.F. Tredgold: Physician specialising in mental diseases. (Who's Who, 1914.)
- Mrs Alec Tweedie: Writer and columnist. (Who's Who, 1914.)
- *Mr W.C.D. Whetham, Senior tutor, Trinity College, Cambridge. Physicist. (Who's Who, 1914.)
F.R.S.:
- Dr Douglas White: Physician. (Medical Directory, 1914.)
- Dr Florence Willey: Lecturer in midwifery, London School of Medicine for Women. (Who's Who, 1914.)

Appendix C

Galton and the Mathematicians: Watson, MacAlister,
Hamilton Dickson, Venn, Burbury and Sheppard

Rev. H.W. Watson, 1827-1903.

Watson studied mathematics at Trinity College, Cambridge, and in 1850 was Second Wrangler.⁽¹⁾ After a brief period as a mathematics teacher, he entered the church, where he continued to pursue his interests in mathematics, which were chiefly in the area of mathematical physics (Watson, 1876; Watson and Burbury, 1879; Watson and Burbury, 1885-9). Galton contacted Watson to help him solve a problem of probability theory that had arisen in his eugenics, that of the probability of the extinction of family names. Watson's partial solution of it (Watson and Galton, 1874) is now regarded as the beginning of the theory of branching processes. D.G. Kendall (1966) discusses it, and describes the subsequent history of the theory. At the time, the 'Galton-Watson process' was taken no further. Watson returned to his own concerns, and did no further work in statistics, apart from one paper (Watson, 1891) in which he discussed a problem, again submitted to him by Galton, to do with the combination of probable errors (for example, in deducing intra-fraternity variability from population variability and/...

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(1) Biographical details are taken from Who was Who 1897-1915 and Bryan (1903).

and the variability of fraternity means).

Galton's collaboration with Watson fell short of what Galton wanted. Writing to Sheppard, Galton commented: 'Watson is over busy and I think too fastidious and timid' (quoted by K. Pearson, 1914-30, 3B, 486-7). It is interesting to speculate how much of Watson's failure to do more work on Galton's problems could be attributed to the cautious attitude to hereditarianism shown by his comment on Hereditary Genius (Watson to Galton, 7 January 1870; Galton Papers, 120/4):

... you do not allow perhaps sufficient importance to the influence of association and surrounding circumstance on the determination of a man's career up to his time of University degree.

Sir Donald MacAlister, 1854-1934.

MacAlister was another Cambridge-trained mathematician, and Senior Wrangler in 1877.⁽²⁾ Like Watson, MacAlister spent a short period as a mathematics teacher before turning to one of the more established professions, in his case medicine. From 1881 he practised and taught medicine in Cambridge. In 1907 he was appointed Principal of the University of Glasgow.

Galton approached MacAlister, whom he first met socially, and set him the problem of finding a 'law of error/...

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(2) For biographical details see the Dictionary of National Biography and E.F.B. MacAlister (1935).

error' for those cases (such as those covered by Fechner's Law) in which the geometric mean was the best measure of central tendency: Galton wanted something corresponding to the normal curve in its relation to the mean. In response, MacAlister produced what has become known as the log-normal distribution (Galton, 1879; MacAlister, 1879). That was, however, his only contribution to statistical theory.

Galton retained a high opinion of him:

He is very favourably disposed towards Eugenics and is, as you know, a vigorous mathematician. (Galton to K. Pearson, 18 August 1910. Quoted in Pearson, 1914-30, 3A, 430.)

MacAlister did help Weldon with his first biometric paper, 'explaining ... many points connected with the law of error' (Weldon, 1890, 445). In general, though, MacAlister's medical career seems to have prevented him from doing as much in the field as he might have wanted. Writing to Galton (2 March 1889; Galton Papers, 279/3) he commented:

Heredity in your hands is becoming fast an exact study. I only wish that my pressing avocations had allowed me to help you more.

J.D. Hamilton Dickson, 1849-1931.

Hamilton Dickson was educated at Glasgow and Cambridge Universities, and was placed Fifth Wrangler in the 1874 Mathematical Tripos.⁽³⁾ In 1877 he was appointed a tutor of Peterhouse College, and he spent most of the remainder/...

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(3) Biographical information is taken from M. McC. F[airgrieve] (1931).

remainder of his life in mathematical teaching and research at Cambridge.

Hamilton Dickson's famous collaboration with Galton is described in chapter two. As was shown there, Galton had in fact virtually solved the problem before handing it over to Hamilton Dickson. In his autobiography Galton mentioned one mathematical collaborator with whom he had particular difficulties of communication because of the divergence (discussed in section 2.2) between the cognitive interests underlying his approach and that of error theory. There is some reason to believe that that mathematician was in fact Hamilton Dickson. A letter to Galton, dated Christmas day 1890 (Galton Papers, 236/4), shows Hamilton Dickson struggling unsuccessfully with the problem of the combination of probable errors solved for Galton by Watson (1891). Hamilton Dickson appears to have been attempting to apply a simple error theory model to a situation in which, as Watson showed, it was inapplicable.⁽⁴⁾ Despite the importance of its first product, Galton's collaboration with Hamilton Dickson thus did not bear further fruit.

John/...

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- (4) Galton (1908, 305) thanks Watson for his help in Galton's 'struggles' with applications of the Gaussian Law, and goes on in the same paragraph to talk of his difficulties with mathematicians failing to comprehend his approach:

I could give a striking case of this, but abstain because it would seem depreciatory of a man whose mathematical powers and ability were far in excess of my own.

John Venn, 1834-1923.

Venn, a member of one of the leading families of the 'intellectual aristocracy', was educated at Cambridge, being placed Sixth Wrangler in the 1857 Mathematical Tripos.⁽⁵⁾ He then entered the Church, but returned to Cambridge in 1862 to become lecturer in moral science at Gonville and Caius College. He remained in Cambridge for the rest of his life, becoming President of his College in 1903.

Venn was, of course, primarily a philosopher, not a mathematician, and his best known work was on symbolic logic and the foundations of the theory of probability. In the 1880's he developed an interest in anthropometry. Little is known of the origins of this interest, but the immediate stimulus to Venn appears to have been a lecture Galton gave in Cambridge in 1884.⁽⁶⁾ Following Galton's lecture a small committee was established to obtain measurements of Cambridge undergraduates similar to those already obtained by Galton in his Anthropometric Laboratory in London. Venn analysed the data gathered on the undergraduates, comparing the physical characteristics of three groups classified according to the class/...

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(5) Biographical information for Venn is taken from the Dictionary of National Biography. For the Venn family see Annan (1955, 276).

(6) See K. Pearson (1914-30, 2, 268) and J. Venn (1888, 140-1). The latter source gives the date of the lecture as June 1885, not 1884.

class of degree they obtained. Venn adapted well-known error theory techniques in a way that was then somewhat unusual to test the significance, as we would now put it, of the differences found.

Venn did do some further statistical work: Venn (1891) discusses non-Gaussian error curves. He did not, however, go beyond these beginnings to make any major contributions to statistical theory.

S.H. Burbury, 1831-1911.

Burbury was trained at Cambridge in both classics and mathematics.⁽⁷⁾ For 20 years after leaving Cambridge he did no scientific work, pursuing instead a legal career. H.W. Watson, who was a close friend of his, reawakened his scientific interests, and together they worked on electromagnetic theory and the kinetic theory of gases (Watson and Burbury, 1879; Watson and Burbury, 1885-9).

At the end of the 1880's, Burbury became interested in problems of the foundations of the kinetic theory. He came to doubt whether the molecules of a gas could be treated as independent from each other in their relative motion. Burbury felt that Galton's theory of correlation provided a possible route to a generalisation of the assumptions underlying the derivation of the theorems of the kinetic theory.⁽⁸⁾
The/...

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(7) Biographical details are taken from Bryan (1911; 1913).

(8) This seems to have been his primary motive for work on correlation, although his letters to Galton (Galton Papers, 212) and membership of the Royal Society's Evolution Committee (K. Pearson, 1906, 289) indicate a certain interest in biometry.

The Maxwell-Boltzmann distribution, derived on the assumption of the mutual independence of velocities, was analogous to the distribution of independent normal variables. Burbury (1894; 1895) modified this distribution by the introduction of product terms, making it analogous to the distribution of correlated normal variables. In doing this he derived, apparently independently of Edgeworth and Pearson, a multivariate generalisation of Galton's bivariate normal surface.

Burbury's approach was sophisticated. In modern terminology we would describe him as having used characteristic functions to obtain a multivariate version of the central limit theorem. The statistician of today would undoubtedly prefer his approach to the problem to that of either Edgeworth or Pearson. Burbury's work was, however, relatively sterile. He attempted to apply his refined model to the problem of the liquefaction of a gas (Burbury, 1899), but was unable to obtain specific quantitative predictions. He did no further work of relevance to statistical theory.

W.F. Sheppard, 1863-1936.

William Fleetwood Sheppard was born in Australia, but sent to England to complete his education.⁽⁹⁾ He won a scholarship to Trinity College, Cambridge and in 1884 was placed Senior Wrangler, ahead of William Bragg. He became a Fellow of Trinity, and published a paper dealing with Bessel/...

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(9) For Sheppard's life and work see N.F. Sheppard (1938), Aitken (1938) and Fisher (1938).

Bessel functions (Sheppard, 1889). He soon left Cambridge, however, and took up a legal career; in 1896 he joined the Education Department (later Department of Education and Science), where he worked until his retirement in 1921.

In his Cambridge days he became interested in Galton's work, and visited Galton's Anthropometric Laboratory several times (W.F. Sheppard, 1938, 3). In the early 1890's he entered into correspondence with Galton (Galton Papers, 245/22 and 315). Galton strongly encouraged him to take up statistical work. Sheppard does not appear to have done so immediately, but in the summer of 1895 he began work on the paper that was to become Sheppard (1898b); by October 1895 he had already reached the main results of that paper (Sheppard to Galton, 8 October 1895; Galton Papers, 315). Galton gave Sheppard considerable help and encouragement, paying for his paper to be typed and negotiating its acceptance by the Royal Society (see the letters between Sheppard and Galton in 1896; Galton Papers, 245/22, 315). By the Autumn of 1896 Sheppard seems to have developed the basic ideas of 'Sheppard's corrections'.⁽¹⁰⁾ Towards the end of the 1890's, the full range of Sheppard's mathematical concerns became clear, with the appearance of a series of papers ranging from/...

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(10) A summary of these appeared as Sheppard (1897b), and they were fully presented in Sheppard (1898c). The first reference to this work is in a letter to Galton of 26 September 1896 (Galton Papers, 315).

from pure mathematics (Sheppard, 1898a) to statistical theory (1899a; 1900) and numerical analysis (1899b; 1899c).

It is not entirely clear why Sheppard took up statistical work. He had fairly wideranging interests in politics (N.F. Sheppard (1938, 3) quotes a description of him as 'a genuine Liberal ... a social reformer of the Toynbee Hall type'), and in culture (Sheppard (1897a) indicates his passion for Wagner). It is possible that the eugenic aspects of Galton's work interested him:

It happens that I have always been interested in 'probabilities', particularly from the logical point of view, and that is the reason why your books have especially interested me as showing their bearing on one branch of the still unsolved mystery of human evolution.
(Sheppard to Galton, 30 October 1892; Galton Papers, 315)

Sheppard's statistical work does not, however, reveal any close connections to eugenic applications.

Appendix D

The Tetrachoric Expansion of the Bivariate Normal Distribution

In this account I have stayed as close as possible to Pearson's original presentation, while removing some of the more detailed steps of the argument. The modern statistician would of course want to improve this account by systematically distinguishing between sample statistics and population parameters. The derivation of the tetrachoric expansion can also be made neater by the use of characteristic functions and Hermite polynomials.⁽¹⁾

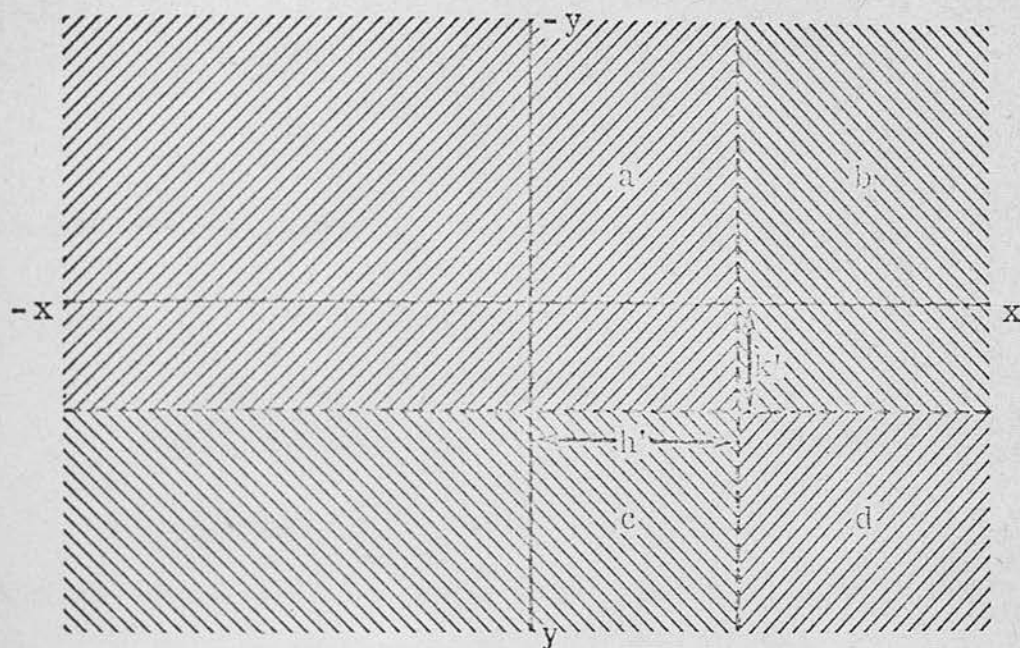
Consider a bivariate normal frequency surface

$$z = \frac{N}{2\pi\sqrt{(1-r^2)}\sigma_1\sigma_2} \exp \left\{ -\frac{1}{2} \frac{1}{(1-r^2)} \left(\frac{x^2}{\sigma_1^2} + \frac{y^2}{\sigma_2^2} - \frac{2rxy}{\sigma_1\sigma_2} \right) \right\}$$

where N is the total number of observations, σ_1 and σ_2 are the standard deviations of variables x and y (both of which are measured in terms of deviations from their respective means), and r is the correlation of x and y. Let this surface be divided into four parts by planes at right angles to the axes of x and y, at distances h' and k' from the origin:

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(1) See M.G. Kendall (1943, 1, 354-6). Note that Kendall's 'Tchebycheff-Hermite Polynomials' (1943, 1, 145-7) are somewhat differently defined from the Hermite polynomials commonly used in applied mathematics (e.g. Arfken, 1968, 477-81).



(This figure corresponds to the horizontal plane of figure 2.)

Let $h = h'/\sigma_1$ and $k = k'/\sigma_2$. Then h and k can easily be evaluated in terms of the frequencies in the four quadrants formed by the two planes. Let these frequencies be a , b , c , d . Then

$$b + d = \int_{h'}^{\infty} \int_{-\infty}^{\infty} z \, dx \, dy = \int_{h'}^{\infty} \left[\int_{-\infty}^{\infty} z \, dy \right] dx.$$

Now
$$\int_{-\infty}^{\infty} z \, dy = \frac{N}{\sqrt{2\pi} \sigma_1} \exp \left\{ -\frac{1}{2} \frac{x^2}{\sigma_1^2} \right\},$$

as this is the unconditional distribution of x .

$$\begin{aligned} \text{So } b + d &= \frac{N}{\sqrt{2\pi} \sigma_1} \int_{h'}^{\infty} \exp \left\{ -\frac{1}{2} \frac{x^2}{\sigma_1^2} \right\} dx \\ &= \frac{N}{\sqrt{2\pi}} \int_h^{\infty} \exp \left\{ -\frac{1}{2} x^2 \right\} dx. \end{aligned}$$

and/...

and h can be evaluated in terms of $b + d$ by use of tables of the normal distribution.

Similarly $e + d = \frac{N}{\sqrt{2\pi}} \int_k^\infty \exp \left\{ -\frac{1}{2} y^2 \right\} dy$

and k can be evaluated in terms of $c + d$.

Now
$$d = \int_{h^-}^\infty \int_{k^-}^\infty z \, dx \, dy$$

$$= \frac{N}{2\pi\sqrt{(1-r^2)}\sigma_1\sigma_2} \int_{h^-}^\infty \int_{k^-}^\infty \exp \left\{ -\frac{1}{2} \frac{1}{(1-r^2)} \left(\frac{x^2}{\sigma_1^2} + \frac{y^2}{\sigma_2^2} - \frac{2rxy}{\sigma_1\sigma_2} \right) \right\} dx \, dy$$

$$= \frac{N}{2\pi\sqrt{(1-r^2)}} \int_{h^-}^\infty \int_{k^-}^\infty \exp \left\{ -\frac{1}{2} \frac{1}{(1-r^2)} (x^2 + y^2 - 2rxy) \right\} dx \, dy.$$

This equation relates r to d , N , h and k (the last two of which we have already evaluated in terms of a , b , c , d), and can be solved for r . If the right-hand side is expanded in a series in r , after some manipulation the following result is obtained:

$$\frac{ad-bc}{N^2hk} = r + \frac{r^2}{2!} h k + \frac{r^3}{3!} (h^2-1)(k^2-1)$$

$$+ \frac{r^4}{4!} h(h^2-3)k(k^2-3)$$

$$+ \frac{r^5}{5!} (h^4-6h^2+3)(k^4-6k^2+3)$$

$$+ \frac{r^6}{6!} h(h^4-10h^2+15)k(k^4-10k^2+15)$$

$$+ \frac{r^7}{7!} (h^6-15h^4+45h^2-15)(k^6-15k^4+45k^2-15)$$

$$+ \frac{r^8}{8!} h(h^6-21h^4+105h^2-105)k(k^6-21k^4+105k^2-105)$$

$$+ \text{etc.},$$

where $H = \frac{1}{\sqrt{2\pi}} \exp \left\{ -\frac{1}{2} h^2 \right\}$ and $K = \frac{1}{\sqrt{2\pi}} \exp \left\{ -\frac{1}{2} k^2 \right\}.$

With/...

With $|r| < 1$, the series converges rapidly, and terms of order higher than r^8 can normally be neglected, leaving a polynomial equation for r that can be solved numerically. Thus, given observed frequencies a, b, c, d it is always possible to fit the model of an underlying bivariate normal distribution to the observations, and to deduce a value for its correlation.

List of Works Cited

The following abbreviations of journal titles have been used:

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|------------------|---|
| J.R.S.S.: | <u>Journal of the Royal Statistical Society</u> |
| Phil. Mag.: | <u>The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science</u> |
| Phil. Trans.: | <u>Philosophical Transactions of the Royal Society of London</u> |
| Proc. Roy. Soc.: | <u>Proceedings of the Royal Society of London.</u> |

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